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# 'FLUCTUATIONS OF ATTENTION' WITH WEAK AUDITORY STIMULI: A STUDY IN PERCEIVING

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This study, the conclusion of an experimental program begun in the Cornell laboratory about a decade ago, carries into the auditory field the procedures that have been successfully applied in the study of 'fluctuations' in the fields of vision[1] and of touch.[2] The structure and function of the visual, tactual, and auditory mechanisms are for the purposes of experimentation ideally varied. The visual apparatus has (a) receptors that are subject to adaptation and (b) intrinsic and extrinsic muscles that are subject to fatigue. The tactual apparatus has (a) receptors which are subject to adaptation but lacks (b), *i.e.* accessory musculature. The auditory apparatus has (b), *i.e.* accessory musculature, but its receptors (a) are not subject to the ordinary phenomenon of adaptation. A comparison, therefore, of the results obtained by comparable methods in these fields should enable us to determine the nature of the 'fluctuations,' *i.e.* whether they are peripheral or central, and whether they depend upon the structure and function of organs involved.

In a study of the visual field, Guilford obtained 'fluctuations' to weak stimuli but he showed conclusively that they could not be ascribed to attention. They were, as he discovered, simply the resultants of the operation of factors effective in all liminal determinations—being partly peripheral and partly central.[3] Freiberg, continuing the investigation into the

* Accepted for publication January 22, 1936. This study was made in the Psychological Laboratory of Cornell University under the direction of Professor Karl M. Dallenbach.

[1] J. P. Guilford, 'Fluctuations of attention' with weak visual stimuli, this JOURNAL, 38, 1927, 534-583.

[2] A. D. Freiberg, 'Fluctuations of attention' with weak tactual stimuli: A study in perceiving, *ibid.*, 49, 1937, 23-36.

[3] *Op. cit.*, 582 f.



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tactual field—with punctiform and areal cutaneous stimuli and with punctiform subcutaneous stimuli—did not obtain 'fluctuations.' He discovered that adaptation was rapid and complete.[4] The present study extends the procedures used in vision and touch to the auditory field.

## Historical Setting

Fluctuations of weak auditory stimuli were the first reported. In 1875, Urbantschitsch, an aurist, discovered,[5] while testing the auditory acuity of some of his patients, that they reported periodic changes in the intensity of the sounds of a ticking watch. The sounds waxed and waned, came and went, even though the distance of the watch from their ears was held constant. Similar results were secured from other faint sounds; a small stream of water and an electrically driven tuning fork yielded fluctuations. Patients with perforated tympana and disconnected ossicles reported the phenomenon; and they were also obtained when bone conduction alone was used. That these fluctuations were not due to physical changes was established by the fact that they did not coincide when several individuals simultaneously observed and reported upon the fluctuations of the same sound. In view of these results, Urbantschitsch concluded that the fluctuations were due to an intermittent sensitivity of the auditory nerve to weak stimuli.

In order to discover more specific characteristics of these fluctuations, Urbantschitsch continued his investigations.[6] He conducted the tone from a large tuning fork to the two ears by means of rubber tubes, which he varied in length until the tone seemed equally intense in both ears. When he struck the tuning fork strongly and conducted this tone to one ear for 10-15 sec., and then partially damped the tuning fork, it was no longer audible. If he now opened the tube leading to the other ear, the partially damped tone was heard in that ear. If, however, he gave either a high or a low tone of strong intensity to one ear, and followed this by a contrasting (low or high) tone of weak intensity to the same ear, the second tone was always heard. When he presented a loud tone to one ear, partially damped the tuning fork and then conducted the sound to both ears, it was heard immediately by the previously non-stimulated ear, and 2–5 sec. later by the 'fatigued' ear, followed by simultaneous disappearance in both ears. Urbantschitsch concluded that the decrease of intensity and the disappearance of the weak tones was due to a rhythmic fatigue of the auditory nerve for a particular pitch region.

In a later investigation,[7] Urbantschitsch used both normal and partially deaf patients. A tuning fork and a watch were employed as sources of energy. Fluctuations were again observed, and in addition a sudden or gradual passing of the tone from one ear to the other. In the gradual passage from ear to ear, the tone first weakened, then disappeared, and finally reappeared. Occasionally a slight noise occurred as the tone passed through the median plane. Urbantschitsch's final conclusion regarding

[4] *Op. cit.*, 36.

[5] Victor Urbantschitsch, Über eine Eigenthümlichkeit der Schallempfindungen geringster Intensität, *Centralbl. f. d. med. Wiss.*, 1875, 625-628.

[6] Urbantschitsch, Zur Lehre von der Schallempfindung, *Arch. f. d. ges. Physiol.*, 24, 1881, 574-595.

[7] Urbantschitsch, Über subjective Schwankungen des Intensität acustischer Empfindungen, *ibid.*, 27, 1882, 436-453.

the fluctuations was that they were not due only to fatigue of the auditory nerve, "sondern sind wenigstens zum Theile den acustischer Sinnesempfindungen selbst eigenthümlich."[8]

Lange was the first to place the phenomenon of fluctuations in a psychological setting.[9] In criticism of the fatigue aspect of Urbantschitsch's theory, he pointed out (1) that there is no experimental evidence that sensory nerves tire so quickly from weak stimuli, (2) that the nerve would have to recover while it was being stimulated, and (3) that we should have more fatigue effects from strong than from weak stimuli—which was not the case. Lange, in his own investigation, used several types of pocket watches and found the same rhythm of fluctuations with all, indicating that the phenomenon was subjective. Measuring from the most intense point of the experience through its disappearance to the peak of the next wave he found a rhythmical fluctuation of about 4-sec. duration, with a mean variation of less than 1 sec. When auditory and visual stimuli were presented together there was neither the auditory nor the visual rhythm, but a new one. This, Lange believed, indicated the importance of central factors. A study of the fluctuations of memory images of the ticking of the watch revealed similar rhythmic fluctuations of slightly shorter duration. Fluctuations were, he concluded, the resultant of a constant sensation being affected by the increase and decrease of the intensity of the corresponding memory images.

These contradictory theories of Urbantschitsch and Lange led to a study by Eckener.[10] As stimuli he used (1) the ticking of a watch, (2) the sound of a stream of fine sand falling on a vibrating steel tongue, and (3) the noise of a telephone receiver diaphragm activated by a rapidly actuated Wagner hammer. Two kinds of fluctuations were reported: (1) 'objective,' which were sudden, complete, short, infrequent, varying in number from *O* to *O*, and occurring when attention was very clear; and (2) 'subjective,' which were gradual, lasted longer, and differed from the 'objective' in that the tone could again be heard by an inner effort. Eckener believed that the first type had its locus in disturbances of the organ of hearing, and that the second type was dependent upon central factors. Fluctuations were obtained from one *O* who lacked a tympanum and two of the ossicles, indicating that peripheral factors were not necessary. A change in the intensity of the noise even when it was continuously heard was also reported. Eckener believed that this refuted the fatigue theory. Cutting off the energy after the *O* had reported the disappearance of the tone often led to a report of some change in the conscious content—indicating the operation of central factors. Memory images of the tones and changes in the limens of the *O*s made experimentation and observation very difficult. The manner of appearance and the duration of the sound was very irregular, and varied from *O* to *O*.

A new approach to the problem of fluctuations was made by Lehmann,[11] who advanced the hypothesis that attention was the resultant of various physiological and psychophysical processes. He used the hiss from a Bunsen burner-flame, investi-

[8] *Idem*, 437.

[9] Nicolai Lange, Beiträge zur Theorie der sinnlichen Aufmerksamkeit und der activen Apperception, *Phil. Stud.*, 4, 1888, 390-422.

[10] Hugo Eckener, Untersuchungen über die Schwankungen der Auffassung minimal Sinnesreiz, *ibid.*, 8, 1893, 343-365.

[11] A. Lehmann, Über die Beziehung zwischen Atmung und Aufmerksamkeit, *ibid.*, 9, 1894, 66-95.

gating the relation between the breathing rhythm and the rhythm of fluctuations. The most favorable occasions for the return of experience were, he found, (1) a short time after the beginning of expiration, and (2) during inspiration. The most unfavorable occasions were (1) at the peak of expiration, when there is the least blood in the brain, and (2) at the height of inspiration, when, although there is an ample supply of blood in the brain, a large amount of neural energy is drained off into the muscles of breathing.

At this point Heinrich started a controversy by reporting that noises fluctuate but that pure tones do not.[12] One *O*, with no tympanum on either side, failed to get intensive fluctuations from the sound of a ticking watch. Heinrich, with normal tympana, secured fluctuations under similar conditions. Again acting as his own *O*, Heinrich reported no fluctuations from pure tones produced by a Galton whistle, an organ, blown bottles, and a tuning fork. Analysis of plethysmographic records showed no correlation between pulse and fluctuations. Pneumographic records, however, indicated some positive correlation between the period of expiration and maximal acuity of hearing, and between inspiration and minimal acuity. On the basis of these results Heinrich concluded that Lehmann had exaggerated the relationship between breathing rhythm and fluctuations.

The startling conclusion of Heinrich that pure tones do not fluctuate in intensity incited Cook[13] to perform a comparatively controlled experiment.[14] He used a Politzer acoumeter of $C^2$, an electrically operated tuning fork of 512 d.v., and a Stern variator of about 256 d.v. An approximation of liminal intensities was obtained by varying the distance from the source of sound to the *O*, and by covering the tuning fork and Stern variator with a box overlaid with cloth. Fluctuations were reported with all stimuli. Cook suggested that a repetition of Heinrich's experiment with more *O*s and with a more careful check to determine that the intensities were liminal might give different results.

Defending his claim that fluctuations were not obtained with pure tones, Heinrich contended that the tones Cook had used were not pure.[15] There was the sparking of the electro-magnet of the tuning fork, and the hiss of the air of the blown bottle. According to Heinrich, another reduction of the purity of the tones was produced by placing both of these sources of sound under wooden boxes covered with cloth. Heinrich repeated his experiment with 3 *O*s, using the tones from singing glass tubes, a blown bottle, and a vibrating steel spring. Every *O* heard the tones continuously, but variations in the intensity of the sound of the spring occurred. Fluctuations were also reported when a Kronecker interrupter was set to give only noises. He concluded as before, that noises fluctuate but that pure tones do not.

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[12] W. Heinrich, Zur Erklärung der Intensitätsschwankungen ebenmerklicher optischer und akustischer Eindrücke, *Bull. internat. de l'Acad. des Sci. de Cracovie*, 1898, 365-381.

[13] H. O. Cook, Fluctuations of the attention to musical tones, this JOURNAL, 11, 1900, 119-123.

[14] One of the routine experiments in the elementary laboratory of psychology at Cornell University had been a study of the fluctuations of sound.

[15] Heinrich, De la constance de perception des tons purs a la limite d'audibilite, *Bull. internat. de l'Acad. des Sci. de Cracovie*, 1900, 37-45.

[16] E. B. Titchener, Fluctuations of the attention to musical tones, this JOURNAL, 12, 1901, 595.

After Heinrich's criticism of Cook, Titchener[16] published a note confirming Heinrich's contention. Titchener used the two lower tones from a gas harmonica (Kohl), running 10 series of observations on 2 *Os*. No fluctuations were obtained.

Without entering into this argument, Wiersma investigated fluctuations of the ticking of a watch.[17] He used (1) normal *Os* in a rested state, after exercise, and under the influence of drugs, and (2) psychopathic patients. Wiersma reported that the proportion of time a sound was heard varied directly with the intensity of stimulus—the louder sounds being heard more of the time than the weak ones.

Another attempt to correlate fluctuations with physiological processes was made by Bonser,[18] who reported fluctuations of sounds produced by a Seashore audiometer. He found that the greatest acuity occurred just after the maximum of vaso-constriction.

Dunlap attempted to check Eckener's conclusion that 'subjective' fluctuations with minimal sounds can be distinguished from 'objective,' and that in 'subjective' fluctuations the actual cessation of the sound can be perceived by *O* even when he is reporting the absence of sound.[19] The current from the secondary element of an induction coil was interrupted by a tuning fork of 100 d.v., and then led off to a telephone receiver. Of 206 interruptions of the sound, 98 were reported. Another experiment resulted in 183 reports out of 319 objective interruptions. Dunlap's *Os* described this phenomenon as the "bottom dropping out," or as a feeling of relief. In another part of his experiment Dunlap used an electric tuning fork of 500 d.v., conducting the tone through a long lead pipe to 4 *Os*. Fluctuations were reported by all of them. An impurer tone of a singing gas flame gave fluctuations, but only after the *Os* had received some training. Dunlap found that after-images of sounds objectively shut off lasted as long as 1.5 min. He concluded that these tended to minimize fluctuations during continuous presentation of a sound. Dunlap came to a conclusion just opposite that of Heinrich; namely, that simplicity, purity and constancy of pitch were favorable to fluctuations, and the lack of these unfavorable. Complex tones seem to fluctuate, he thought, because the *O* was listening to one component.

At about the same time Hammer resurrected the idea that fluctuations of sound were due to variations in the physical sources.[20] He used an electro-magnetic hammer activated by a current of 100 volts, interrupting the current with a metronome. No fluctuations were reported by the 5 *Os*. Hammer concluded therefore that sound did not fluctuate, and that the fluctuations occurring in other sense departments were extra-attentional.

Hammer's article elicited a spirited reply from Seashore,[21] who criticised Hammer's technique, and also reported an experiment using the Runne-chronometer as a source of sound. All of his 55 *Os* reported fluctuations. The argument did not, however,

[17] Enno Wiersma, Untersuchungen über die sogenannten Aufmerksamkeits-schwankungen, *Zsch. f. Psychol.*, 26, 1901, 168-200; 28, 1902, 179-199; 31, 1903, 110-128.

[18] F. G. Bonser, A study of the relations between mental activity and the circulation of the blood, *Psychol. Rev.*, 10, 1903, 120-138.

[19] Knight Dunlap, Some peculiarities of fluctuating and of inaudible sounds, *ibid.*, 11, 1904, 308-317.

[20] Bertril Hammer, Zur experimentellen Kritik der Theorie der Aufmerksamkeits-schwankungen, *Zsch. f. Psychol.*, 37, 1905, 363-376.

[21] C. E. Seashore, Die Aufmerksamkeitsschwankungen, *ibid.*, 39, 1905, 448-450.

end here. Hammer replied,[32] insisting that even the Runne-chronometer might show variations in output. In the same article he reported the results of a study with one *O* in which the source of sound was a 100 d.v. tuning fork, driven by a constant electric source. Though the sound was reduced to minimal intensity, no fluctuations occurred.

The first observation of the uniformity of physical output was made by Jackson.[33] He mounted a lever on the diaphragm of a telephone receiver. On the end of this lever, which gave a threefold amplification, he fastened a small mirror. The rays of light reflected from this mirror were observed through a telescope with a micrometer eye-piece. Jackson was able, therefore, to keep a constant check upon the amplitude of the vibrations. A Koenig tuning fork of 50 d.v. was used to break the circuit containing a variable resistance and a milliammeter. The amplitude was usually constant, only single, long oscillations occurring. There were no regular changes of intensity corresponding to the fluctuations reported by the *O*s, 6 in number. All reported (1) that the sound was heard, (2) that the sound was seemingly lost in a confused mass of sensations (the *O*s felt that the sound was there, but were quite uncertain), and (3) that the sound, when it returned, seemed to emerge out of this confused mass. Fluctuations were hard to obtain, even after training, and occurred only within a narrow range of intensities.

The argument came to a close without being settled by another contribution from Heinrich.[34] He used a Kronecker interrupter adjusted to give a tone accompanied by a noise. One *O* reported that the tone remained constant, but that the noise disappeared periodically. Heinrich brought in evidence from physiology that within limits the tensor tympani reacts to tones, regardless of intensity, whereas a very fine tuning is required for noises, particularly very weak ones. He added the assumption that the tensor tympani shows rhythmic changes of tension. This was, he thought, the mechanism underlying the fluctuations of noises and the constancy of pure tones.

*Summary and discussion.* The studies reviewed above are in general inadequate or deficient in two respects: first, in their experimental control; and secondly, in their interpretation of their results.

None of the investigators knew exactly the wave-form of his stimuli, and only one of them, Jackson, made any serious attempt to control this variable. It is not surprising, therefore, that arguments and controversies should have arisen concerning the purity of the stimulus-tones.

Although various attempts were made by the different investigators to control and to vary the intensity (wave-amplitude) of their stimulus-sounds, none of them was entirely adequate. The first method used of obtaining different intensities was to change the distance of *O* from the source of sound. This was soon replaced by conducting the stimulus-sounds through rubber tubes and through pipes of different lengths, by varying the force of the air currents actuating blown bottles, by varying the voltage of electrically driven tuning forks, by masking or muffling the source

[32] Hammer, Zur Kritik des Problems der Aufmerksamkeitsschwankungen, *ibid.*, 41, 1906, 48-50.

[33] G. L. Jackson, The telephone and attention waves, *J. Phil., Psychol., & Sci. Methods*, 3, 1906, 602-604.

[34] Heinrich, Über die Intensitätsänderung schwacher Geräusche, *Zsch. f. Sinnesphysiol.*, 41, 1907, 57-58.

of the sound with cloths, pads, blankets, or semi-soundproof boxes. All of these methods were at best rough and inaccurate; none of them gave a precise picture of the stimulus-intensities used. With the exception of Jackson, none of the investigators measured the amplitude of his stimulus-sounds, and Jackson's measures were far from satisfactory. Furthermore, none of the investigators varied the output of his stimulus-sounds in standard units that could be duplicated in other studies.

Eckener's discovery that changes in *O*'s auditory acuity are frequent and marked, and Jackson's discovery that the range of auditory intensities giving fluctuation is very narrow—results that have for the most part been forgotten or ignored—aggravate and intensify the problem and necessitate the exact definition of the stimulus-sounds employed. Statements that "weak and liminal sounds" were used will not suffice. We must know precisely what intensities were employed, the extent and range of their variation, and the precision of their control.

Furthermore, the experiments in none of the investigations were conducted in a soundproof room. Several of the authors mentioned that they conducted their experiments under comparatively quiet conditions—in rooms with closed doors, early in the morning, late at night—but anyone who has studied weak sounds under such conditions knows that the auditory field is filled with distractions that might well produce fluctuations. It seems strange that men who insisted upon darkrooms for visual studies would be content to conduct these auditory studies under such adverse conditions.

A further criticism of the earlier studies is that none of them gave an adequate description of the fluctuations. Urbantschitsch and Heinrich made no attempt to do this. They were more interested in determining the locus of the fluctuations, which they finally placed in the peripheral mechanism of hearing. Heinrich, to be sure, reported a correlation between the fluctuations of a noise and the breathing rhythm, but he never touched the problem of description. Similarly with the other experimenters. Lehmann and Bonser were primarily concerned with the physiological basis. Lehmann attributed fluctuations to the effect of the breathing rhythm on the level of blood supply and neural energy in the brain, and Bonser correlated fluctuations with vasomotor changes. Hammer, on the other hand, was concerned with their physical basis. He suggested that fluctuations were due to irregularities in the physical source of sound—an hypothesis which had already been demonstrated to be fallacious. A few experimenters did, however, offer a psychological account. Lange, the first to attach psychological significance to the phenomenon, believed that fluctuations were periodicities of consciousness caused by the fluctuations of an active apperception. Eckener, without being explicit, ascribed fluctuations to changes in psychic content. Strangely enough, neither Titchener nor Cook offered a psychological account.

The failures of the earlier investigators are due primarily to the apparatus-limitations of their day and generation. They lacked soundproof rooms and apparatus capable of producing auditory stimuli of known wave-form and amplitude. Because of the recent advances in physics and radio-engineering, most of their failures and errors of omission can now fortunately be avoided or corrected. The inadequacy of the psychological account was not, however, due to factors out of the reach of the men who did the experimenting. The neglect of that aspect is as unexplainable as it is surprising.

## PROBLEM

Our investigation had three aims. We wished (1) to discover whether pure and complex tones of weak intensities fluctuated; if fluctuations occurred, we desired (2) to determine the factors which condition them; and we hoped (3) to obtain a psychological account of the fluctuations from our *O*s.

## METHOD

*Apparatus.* The experiments were conducted in a soundproof room.[25] *O* sat in a chair to which an adjustable biting-board was attached, and on the arm of which a make and break key was mounted. This key, in circuit with an electro-magnet in *E*'s room, was made to operate noiselessly by a mercury cup—the mercury being covered with a film of light oil to prevent sparking. A further precaution against sparking was the use of a 0.3-mfd. condenser connected across the binding posts of the key. *E* signalled *O* by means of a small red light, which also served as a source of illumination while *O* was writing his reports.

In order to eliminate the problem of standing waves we presented the tones by ear-phones. They were Western Electric, Signal Corps, Type P-11, 2000 ohms resistance each. Sponge rubber ear cushions made the phones more comfortable.

*E*'s room was adjacent to the soundproof room. A beat oscillator, energized by 110 volts A.C. was our source of sound. It had a calibrated pitch range of 0–15,000~, and a sinusoidal output ranging from 0-6.5 volts. The plate current was stabilized by 135 volts, and the grid current by 45 volts from B-batteries. The output of the oscillator was passed through an attenuation unit designed for the purpose. Its first stage was a mixing transformer of 200 ohms impedance. From the secondary of the transformer the current passed through three attenuators in series, so wired that any one or all of them could be eliminated from the circuit. The desired voltage of the secondary of the transformer was maintained by means of the control in the last stage of the oscillator. The voltage from the secondary of the transformer was measured by a Weston A.C. voltmeter. A single earphone was connected immediately beyond the voltmeter so that *E* could listen to the tone and detect any changes which might occur. One of the attenuators (A) was calibrated in 5-db. (decibel) steps and gave a maximum drop of 45 db.; the other two (B and C) were calibrated in 1.5-db. steps, (B) giving a maximum drop of 43.5 db. and (C) a maximum drop of 30 db. Altogether, therefore, we had a drop of 118.5 db. Each attenuator had a constant impedance of 200 ohms. The final stage in the attenuation was two wound-wire potentiometers having an impedance of 400 ohms each. The output from one potentiometer went to the right ear-phone, the output from the other to the left. This enabled *E* to present tones independently to either ear, or to both ears simultaneously. Shielded, rubber-jacketed cable conducted the current from the potentiometers to the ear-phones. One side of the attenuation circuit was grounded to eliminate induced currents.

A Renshaw polygraph was used for the recording device. One pen was attached to *O*'s signal-key and a second one, for a time-line, to a clock set for seconds.

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[25] For a description of the soundproof room see K. M. Dallenbach, The psychological laboratory of Cornell University, this JOURNAL, 43, 1931, 295-300. The room has been lined with ozite, and rugs have been placed on the floor, to reduce standing waves.

*Procedure.* We used tones of three frequencies: Tone 1, 261~; Tone 2, 896~, Tone 3, 1280~. Fig. 1 shows the wave-form of these tones. Tone 1 has a comp'ex wave-form due to resonance of the diaphragm of the ear-phones at this frequency. The lower tracing in Fig. 1a is of the tone going into the ear-phones, and the upper tracing is of the tone coming out of the ear-phones. This distortion of the wave-form disappeared at about 540~, so pictures of only the output of Tones 2 and 3 were taken. The lower tracings in Fig. 1b and 1c are of a time-line of 60~. Tones 2 and 3, as the figure indicates, are pure tones. Koenig tuning-forks were used in

a. Tone 1, 261~ b. Tone 2, 896~ c. Tone 3, 1280~

FIG. 1. OSCILLOGRAPHIC RECORDS SHOWING THE WAVE-FORM OF THE STIMULUS-TONES

adjusting the oscillator to the desired pitch. Before any observations were run, the oscillator was operated until there was no appreciable 'drifting.'

## PRELIMINARY EXPERIMENTS

Our first task was to determine our *Os*' intensive limens for every one of these tones. It was not sufficient that we know the intensity of the stimuli used in this experiment; we had to know what relation the intensities used had to our *O*'s limens.

*Method and procedure.* A combination of the discrete and continuous method of limits was used in the liminal determinations. We selected 0.275 volts as an arbitrary level from which to measure the db. drop. From this base level of 0.275 volts an appropriate db.-drop was subtracted by means of Attenuators A and B, so that the place of disappearance and appearance of the tone in the descending and ascending series was indicated on Attenuator C at a point about half way from each end of the dial. The intensities of the tones were varied by means of Attenuator C at the rate of 1.5 db. every 2 sec., one second of which was used in turning the dial one step. A metronome beating 60 times per min. was used in determining this rhythm. Three series of 20 presentations each (10 descending and 10 ascending) were given every experimental period: one series to one ear, one to the other, and a third to both ears simultaneously. A 3-min. rest period was given after every series. The initial setting of Attenuator C was varied from presentation to presentation in a planned haphazard order. The order of presentation of the tones and the sequence in which the ears were stimulated also followed the planned haphazard order.

*Observers.* The *Os* were J. D. Coakley (*C*), W. S. Neff (*N*), and T. A. Ryan (*R*) —graduate students and assistants in psychology. All of them were experienced in auditory observation. *N* had a large number of subjective tones or tinniti, which made perception of the weak tones difficult and unstable.

*Instructions.* At the beginning of every experimental period, *O* seated himself in the chair, adjusted his biting-board, put on the ear-phones, and then read the following instructions.

The turning off of the light is the signal for attention. Shortly after this, one flicker of the light will be given indicating the presentation of (1) an audible tone which will gradually become inaudible—depress the key as soon as you cease to hear it; or (2) an inaudible tone which will become audible—depress the key as soon as you hear it. Maintain a relaxed and passive attitude throughout the experimental period.

Every *O* was given a practice series of experiments so that he would become accustomed to the apparatus and familiar with weak tones. The test experiments, consisting of 100 series (50 descending and 50 ascending) for every one of the three tones and for each ear alone and for both ears together (900 series for every *O*), were then conducted. The limens for every O for the right (R), left (L), and for both (B) ears were computed by the method of limits.

*Results. (1) Intensive limens.* Table I shows the limens obtained for every *O*, for all the tones, for each ear alone, and for both ears together in terms of db.-drop from the base level of 0.275 volts. Since the figures express

TABLE I

LIMENS FOR EVERY *O* IN DECIBLE-DROP FOR TONES 1, 2, AND 3, FOR BOTH EARS TOGETHER AND FOR THE RIGHT AND THE LEFT EARS SEPARATELY

| | *O* | Ear | Av. Limen | Range | m.v. |
|---|---|---|---|---|---|
| | | B | 54.33 | 49.5–60.0 (10.5) | 1.65 |
| | *C* | R | 55.83 | 51.0–60.0 ( 9.0) | 1.34 |
| | | L | 48.75 | 43.5–55.5 (12.0) | 1.85 |
| | | B | 61.43 | 55.5–66.0 (10.5) | 1.63 |
| Tone 1, 261∼ | *N* | R | 64.88 | 60.0–75.0 (15.0) | 2.89 |
| | | L | 58.73 | 52.5–67.5 (15.0) | 2.29 |
| | | B | 51.63 | 44.5–59.5 (15.0) | 2.74 |
| | *R* | R | 52.47 | 47.5–56.5 ( 9.0) | 1.85 |
| | | L | 50.52 | 46.0–58.0 (12.0) | 2.13 |
| | | B | 80.00 | 76.5–87.0 (10.5) | 2.34 |
| | *C* | R | 72.08 | 67.5–78.0 (10.5) | 1.65 |
| | | L | 73.88 | 69.0–79.5 (10.5) | 2.48 |
| | | B | 90.02 | 80.0–99.5 (19.5) | 3.18 |
| Tone 2, 896∼ | *N* | R | 80.08 | 69.5–90.5 (21.0) | 3.13 |
| | | L | 87.76 | 75.5–98.0 (22.5) | 3.50 |
| | | B | 79.69 | 73.5–85.5 (12.0) | 1.64 |
| | *R* | R | 69.47 | 66.0–72.0 ( 6.0) | 1.29 |
| | | L | 77.33 | 70.5–84.0 (13.5) | 2.58 |
| | | B | 68.84 | 63.5–72.5 ( 9.0) | 1.63 |
| | *C* | R | 65.92 | 62.0–69.5 ( 7.5) | 1.72 |
| | | L | 65.45 | 62.0–71.0 ( 9.0) | 1.56 |
| Tone 3, 1280∼ | | B | 68.76 | 64.5–72.0 ( 7.5) | 1.35 |
| | *R* | R | 66.51 | 61.5–72.0 (10.5) | 2.30 |
| | | L | 61.26 | 57.0–69.0 (12.0) | 2.21 |

Liminal determinations were also attempted with *N* for Tone 3, but very distracting tinniti of about the same pitch made these determinations very irregular and unreliable. They are therefore not included in this table.

a drop from a constant voltage level, the larger they are the weaker the tone. When the points of disappearance and appearance of the tones in the descending and ascending series are plotted with db.-drop along the abscissa and percentage along the ordinate, we get the familiar bell-shaped curve of distribution. *N* shows a larger scatter of judgments than *C* or *R*, probably because of his tinniti.

In these experiments, which were undertaken to determine the intensive ranges of our stimuli, we found: (1) that the limen depends upon the pitch of the tone—it is lowest for Tone 2 (896~) and highest for Tone 1 (261~); (2) that individual differences are marked; and (3) that large differences exist between the two ears, and between each of them alone and both of them together. The results for Tone 2 show a definite increase of acuity when both ears are stimulated simultaneously. This is less strikingly true for Tone 3. Tone 1 does not, however, show this advantage for the two ears.

## Main Experiments

After calibrating the apparatus and determining the sensory limens of our *O*s for both ears together and for each ear separately, we turned to the main problem.

*Procedure.* The routine, before presenting a tone to *O*, was as follows: the pitch of the oscillator was checked; the voltage of the secondary coil of the transformer was brought to the base-level of 0.275 volts; the polygraph, upon which *O*'s reports of fluctuation were recorded, was started; and the light, which served as *O*'s ready signal, was turned off. A tone (Tone 1, 2, or 3) was then presented to *O*, at first and for brief interval at a weak but clearly audible level so that *O* would know the pitch-range to which he was to attend, and then, after the attenuators had been adjusted to the intensity-level desired, for an interval of 3 min. At the end of this time, the signal light was turned on and the pitch of the oscillator was again checked.

The intensity-values that we used at first were selected on the basis of the *O*s, limens: one value was at or near the limen and two were above and two were below it. We soon discovered, however, that these values were too high. The tones did not fluctuate, they were reported as present during the entire 3-min. period of the experiment. We had, therefore, to select weaker intensities. We did this by starting with a tone that was audible all the time and then by descending in steps of 1.5 or 3 db. until we reached a level that was never audible.

We thought at first to use the haphazard order in stimulating the *O*s' ears, and in presenting the various stimulus-intensities to the *O*s. We soon discovered, however, that we could not do this. The haphazard variation of all of these moments could not be completed in a single experimental period, and we could not complete it at a later period because of the large hour-to-hour and day-to-day shifts in auditory acuity. We decided, therefore, to present one tone to one ear (or to both ears simultaneously) during a single experimental period, and to vary the intensity of that tone until the range yielding fluctuations had been covered.

*Observers.* Various difficulties were encountered with the *Os* during the course of the experiments. *N*'s tinniti continued to render his observations difficult. *R*, shortly after the completion of the preliminary experiments, contracted an infection in the eustachian tubes and middle ears which gave rise to numerous auditory disturbances, ranging from roaring and rushing sounds to the more common forms of tinniti. His observations had, therefore, to be postponed for some time. *C* became interested in the psychological criteria of tone. The shift in attitude resulting raised his limens and reduced the number of his fluctuations. The difficulty in his case was easily corrected once it was discovered and pointed out, but not so the difficulties with the other two *Os*. We enlisted, therefore, the services of two other *Os*: Miss M. B. Erb (*Er*), and Miss M. M. Shaw (*Sh*), both graduate students in psychology. Their intensive limens were not determined. They were, upon the basis of the results of the other *Os*, given a tone and the intensity in subsequent experiments was raised or lowered as required until the liminal range was completely covered. Tone 2 and 3 were used with *Er*, and Tones 1, 2, and 3 with *Sh*.

*Instructions.* At the beginning of every experimental period, *O* was given the following instructions.

Shortly after the light is turned off a clearly audible tone will be given momentarily. This will be followed by a flicker of the light, and then by a tone of the same pitch as the one presented momentarily. Listen attentively, depressing the key while you hear it, and releasing the key when you do not hear it. When the light is turned on write down the location, the characteristics of the tone, the cues used in hearing it, and any self-instruction occurring during the observation. Depress the key momentarily when you are ready for the next presentation.

*Results: Fluctuations.* All the *Os* reported fluctuations of the pure tones (Tones 2 and 3) as well as of the complex tone (Tone 1), whether one or both ears were stimulated. This result carries us immediately to the second object of our study, which was, it will be recalled, concerned with the factors which condition the fluctuations.

*(1) Conditions: (a) Wave-form.* Two of the conditions which have been considered by previous investigators to be important determiners of fluctuations are (1) the intensity of the stimulus, and (2) the nature of the wave-form used. Investigators have found, in general, that only approximately liminal (weak) tones or noises fluctuate. There has been less agreement concerning the nature of the wave which determines fluctuations. Due to the fact that our ear-phones showed a resonance point at about the frequency of Tone 1 (261∼), the wave-form of this tone was complex (see Fig. 1a). Tones 2 and 3, however, were sinusoidal. Our *Os* reported fluctuations, however, with all three tones.

*(b) Intensity.* Fig. 2 shows the range of intensities that yield fluctuations for every *O*, for every tone, and for B, R, and L ears. Every perpendicular line shows the range of intensities used during a single experiment: the lower end indicates the db.-drop which gave audibility 100% of the time, and the upper end the intensity level that was *just not* heard

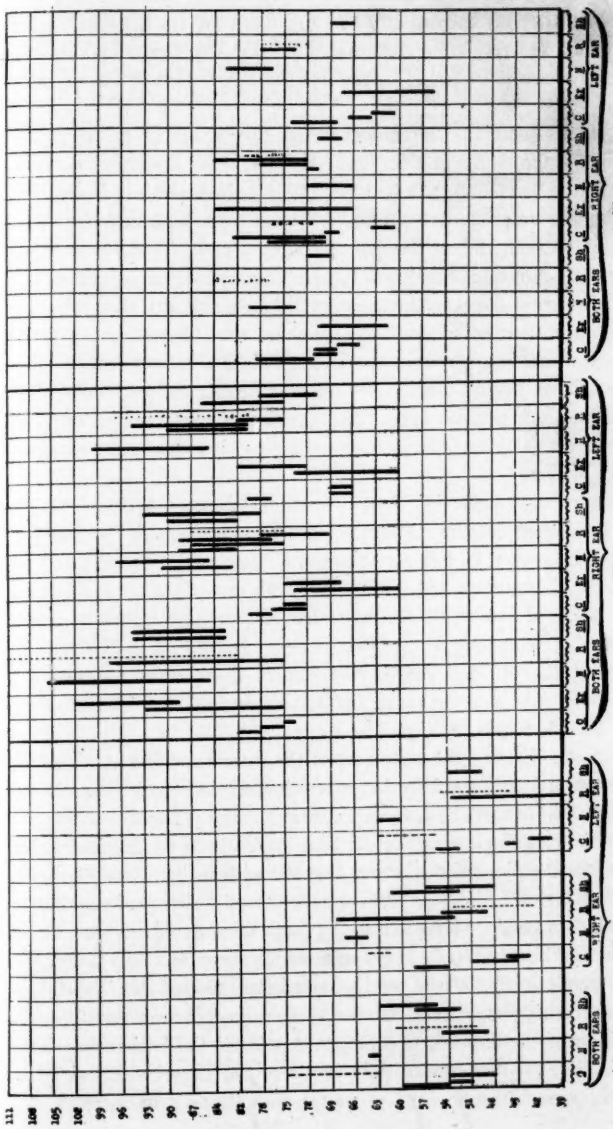



FIG. 2. RANGE OF INTENSITIES YIELDING FLUCTUATIONS

Every line shows the ranges of intensity that was heard by the *Os* between 1% and 100% of the time. The lower end indicates the db.-drop that gave audibility 100% of the time during the 3-min. experimental period, and the upper end the intensity level that was heard 0% of the time.

any of the time. The broken lines for *C* are for the observations that were made by him after his shift in attitude, and for *R* are for the observations made after his recovery from the aural infection. The chronological order of the experiments is indicated by the left-right arrangement for any tone, ear, and *O*.

These results show the differences in auditory acuity of the B, R, and L ears among the different *O*s. They stand in close agreement to those obtained in the liminal determinations. The db.-drop yielding fluctuations varied with the tone: Tone 2 (896∼) required the least intensity, Tone 3 (1280∼) an intermediate degree, and Tone 1 (261∼) the most. This, again, is the relationship obtained in the preliminary liminal determinations.

Fig. 2 also shows, in corroboration of Eckener's results, that there was a marked shift in our *O*s' auditory acuity on the different experimental days. These shifts sometimes covered db.-changes that were greater than the ranges giving fluctuations. This result was further complicated by occasional shifts during a given experimental period. These short-period variations in the *O*s' acuity will be considered below; it suffices here merely to make note of them.

The results shown in Fig. 2 also confirm Jackson's finding that the range of intensities yielding fluctuations is at times very small. For example: Tone 1, *N*, both ears, *C*, left ear; Tone 2, *C*, both ears, right ear, and left ear; Tone 3, *C*, both ears and left ear, *Sh*, both ears, right ear and left ear. In these cases, a very small db.-drop changes the tone from one that is heard 100% of the time to one that is never heard. Table II shows the frequency of the db-drop ranges that yielded fluctuations for our three stimulus-tones. The average range for Tone 1 is 6.96 db.; Tone 2, 10.54 db.; and Tone 3, 5.9 db.

*(c) Attitude.* During the early part of the main experiments, *C*, who is musically trained and specially interested in audition, took advantage of the opportunity offered by our experiment to observe and to study the characteristics of liminal tones. While laboring under these self-instructions —he had no intention of neglecting his task with us but thought that he could carry forward the two tasks simultaneously—he gradually set up criteria which necessitated greater and greater amounts of physical energy. As shown for him in Fig. 2 the range the db.-drop yielding fluctuations was successively lower. When we discovered the cause of this "drift," and set him about his proper task, there was a marked increase in his acuity. The position of the db.-drop yielding fluctuations—the broken line in his records in Fig. 2—rose immediately in all of the experiments with him. The importance of attitude, for *C* at least, is thus clearly demonstrated.

*(2) Relationship of db.-drop to percentage of time heard.* Guilford found that "fluctuations of minimal sensations are a phenomenon of the stimulus limen."[26] If this holds for auditory stimuli we would expect to find a definite relationship between a particular db.-drop and the percentage of time that tone was heard. Fig. 2 shows, however, that a general relationship of absolute intensity to percentage of time heard cannot be established. The day to day overlap of the ranges of intensities giving fluctuations would smooth out or eliminate entirely the actual relationship found for one experimental period. In order to eliminate the differences between ears, *Os*,

TABLE II

RANGES OF INTENSITY IN DB-DROP YIELDING FLUCTUATIONS

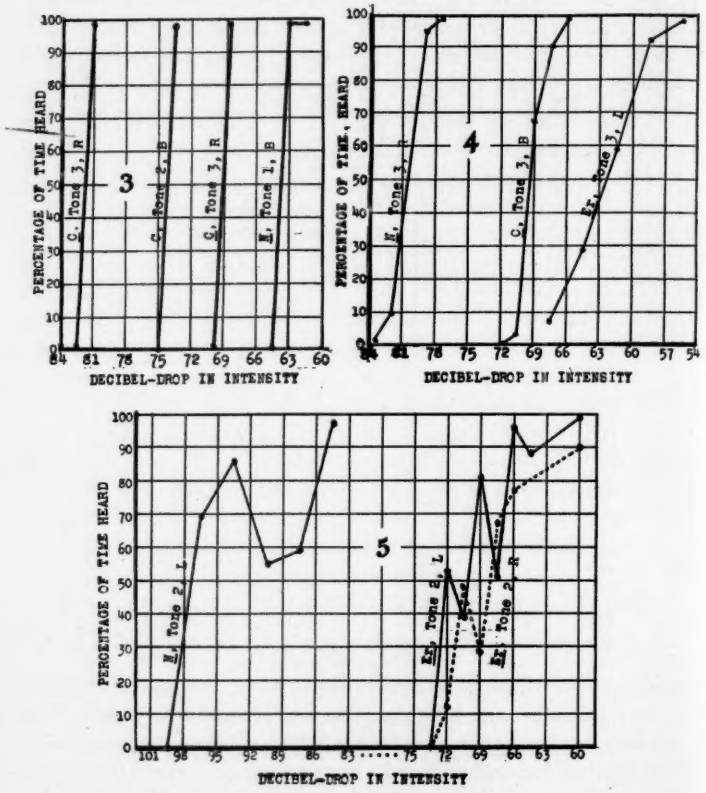
(Results of all the *Os* and ears combined)

| Range (in db.) | Frequence of occurrence | | |
|---|---|---|---|
| | Tone 1 | Tone 2 | Tone 3 |
| 1.5 | 2 | 1 | 2 |
| 3.0 | 7 | 7 | 10 |
| 4.5 | 2 | 1 | 1 |
| 6.0 | 6 | 1 | 8 |
| 7.5 | 2 | 3 | 3 |
| 9.0 | 3 | 4 | 1 |
| 10.5 | 2 | 2 | |
| 12.0 | 1 | 6 | 3 |
| 13.5 | | 3 | |
| 15.0 | 2 | 3 | |
| 16.5 | | | |
| 18.0 | | 2 | 1 |
| 19.5 | | | |
| 21.0 | | 1 | |
| 22.5 | | 1 | |
| 30.0 | | 1 | |
| No. | 25 | 36 | 29 |
| Av. db.-range | 6.96 | 10.54 | 5.9 |

and day-to-day variations of acuity, we plotted curves showing this relationship for a particular tone for one ear for one experimental period. These curves may be divided into three general types. Type 1 is illustrated in Fig. 3. Here we find that a tone of a certain db.-drop is heard throughout, but the same tone with a drop of a single step—1.5 db.—is not heard at all. This type was most common for *C* while he was observing under the "tonal" attitude. The second type, Type 2, is illustrated in Fig. 4. Here we find an approximation of phi-gamma curve. There is a definite co-variability between intensity of the stimulus-tone and the percentage of time that the tone is audible. The third type, Type 3, is shown in Fig. 5. There is a

[26] *Op. cit.*, 554.

gross relationship between intensity and percentage of time heard, but there are many irregularities and inversions of this relationship. These irregularities and inversions seem to be due (a) to momentary shifts in auditory acuity, and (b) to changes in tinniti.




FIGS. 3-5. CURVES ILLUSTRATING THE VARIOUS TYPES OF RELATIONSHIP THAT EXISTS FOR OUR OS BETWEEN STIMULUS-INTENSITY AND PERCENTAGE OF TIME HEARD

Fig. 3 shows curves of Type 1; Fig. 4, of Type 2; and Fig. 5, of Type 3.

These curves vary, then, from those showing a change from inaudibility to audibility over a range of 1.5 db. to those showing numerous inversions of the relationship between intensity and percentage of time heard. Between

these three arbitrary types selected there are many intermediate types. Despite these differences in the forms of the curves we may conclude that there is a definite relationship between intensity of stimuli of approximately liminal value and percentage of time heard. In order to obtain a generalized and reliable curve showing this relationship it will be necessary to run a large number of observations with *O*s who are free from tinniti.

*(3) Relationship of frequency of fluctuations to percentage of time heard.* If Guilford's contention that fluctuations are due to the same factors which determine the limen is true, we should expect the greatest number of fluctuations at those intensities heard about half of the time, and a decrease in the number of fluctuations as one approached 0% or 100% audibility.

If we had been able to show a generalized relationship between percentage of time a tone was heard and its intensity, we could have plotted the number of fluctuations during a 3-min. experimental period directly against intensity. We could not, however, do this because of the day-to-day and short-period variations of the *O*s acuity. In order, therefore, to obtain a picture of the relationship, we compared similar fractions of the audibility-curves of our *O*s. That is, we plotted the number of fluctuations against the percentage of the 3-min. experimental period that a tone was heard. In this way we avoided differences in sensitivity of the ears, differences of acuity among our *O*s, and the dependency of liminal intensity upon the tone used. As a measure of the number of fluctuations, we used the number of times a tone was heard during the 3-min. experimental period. For ease of computation and presentation of the results, we used decile-groupings.

Fig. 6 shows for every *O* the relationship of the number of fluctuations to percentage of time heard. Every curve is based on the results from all the tones, and from B, R, and L ears. Curve A, for *C*, was constructed from data obtained under his self-imposed instructions—when he was set to observe the attributes of tone. He reported very few fluctuations under this attitude. The tones were for the most part either heard for the entire period of the experiment or not heard during any part of it. Curve B was constructed from data obtained after his attitude had been changed. Fluctuations were now more numerous and their curve plotted against percentage of time heard is fairly regular and uniform. Its deviations, particularly those in the upper decile-groupings, are probably due to the small number of cases involved.

Curve C (Fig. 6) is constructed from data that was secured from *R* during the early part of the study before his aural-infection became so serious that experimenting with him had to be temporarily given up. Curve D is based on data obtained after he had recovered from the in-

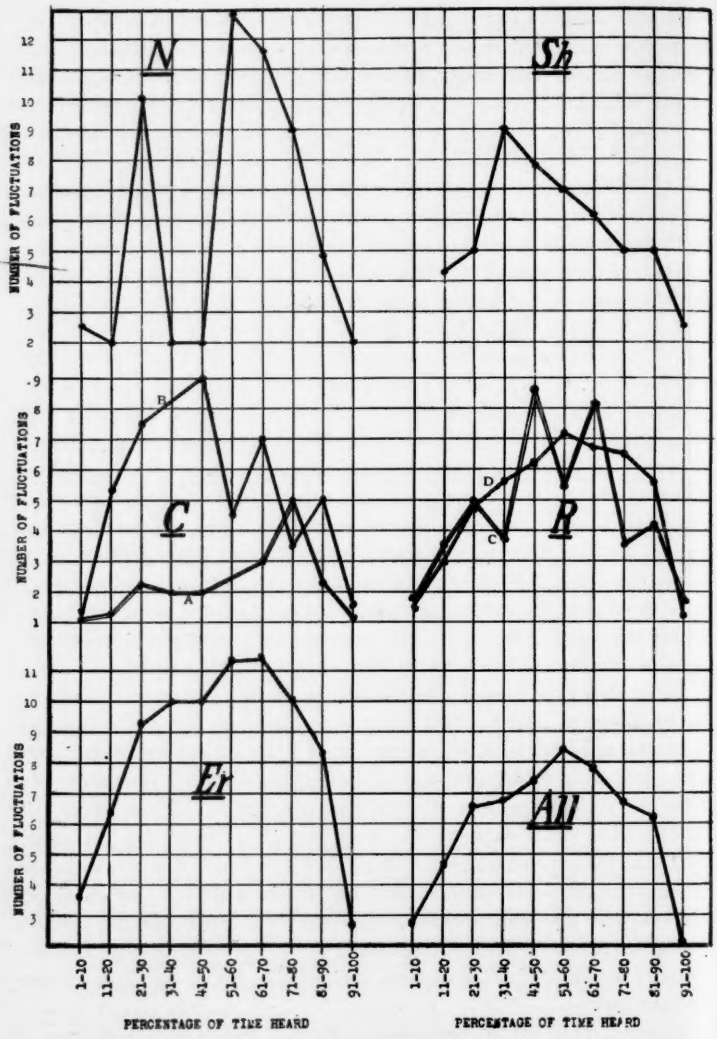



FIG. 6. CURVES SHOWING THE RELATIONSHIP BETWEEN THE NUMBER OF FLUCTUATIONS AND THE PERCENTAGE OF THE 3-MIN. EXPERIMENTAL PERIOD THAT THE TONE WAS HEARD

fection. *N's* curve shows a pronounced inversion at the 31-40, and 41-50 percentiles. This may be due to the meagerness of the data—the point in each case was determined by the results of but one experiment. All the other points on his curve are smooth and regular.

*Sh's* curve shows a striking skewness for which we have no explanation. *Er* was, it will be recalled, bothered but little by tinnitus. Her curve, based on the results from Tones 2 and 3, clearly indicates the greater instability of perception when the tone is of such an intensity that it is heard about half of the time. A generalized picture of the relationship between frequency of fluctuation and percentage of time heard is shown in the last curve in Fig. 6, which was constructed from the combined results of all our *O*s. *N*'s results, *R*'s results secured while he was suffering from the aural infection, and *C*'s results secured while he was observing under his first attitude, were not included among the data from which this curve was plotted.

There is a definite relationship between intensity of tone and rate of fluctuation. The most rapid fluctuations occur at intensities what are heard approximately half of the time, and there is a decrease in the rate of fluctuation as 0% or 100% audibility is approached. That is what would be expected if the hearing of the tone depended upon a large number of factors operating in chance fashion, since the balance of factors would be most easily upset where they were divided approximately equally for audibility and inaudibility. This result confirms Guilford's finding in the visual field.[27]

*(4) Auditory adaptation.* The protocols of the *O*s indicated that the tones sometimes seemed to decrease in intensity during the latter part of the 3-min. period. In order to discover whether there was a corresponding decrease in the percentage of time heard as indicated by the polygraph records, we divided the period into three intervals of 1 min. each. Since those tones which were reported present all of the time would not show any difference between the three 1-min. periods, we limited our analysis to those heard 95% of the time or less. Table III shows the results of this analysis. When we group all tones to discover tendencies for individuals we find that: *C* showed a slight decrease in percentage of time heard as the 3-min. period progressed; *Er* and *N* show a slight increase in the second minute; and *R* and *Sh* seem to do better in the first and third minutes than in the second. These slight and unreliable individual differences cancel out, however, when we average the results for all *O*s for all tones. Our results show: 53% audibility for the first minute; 52% audibility for the second minute; and 53% audibility for the third minute. As Table III indicates, similar results were obtained when the tones were considered separately. We conclude, therefore, that, under the conditions of our experiment, auditory adaptation does not occur.

*(5) Effects of tinnitus.* One is ordinarily unaware of the existence of tinniti, but they become clearly audible in a soundproof room where they are not masked by

[27] Guilford, *op. cit.*, 554.

outside noises. Of our three original *O*s, two (*N* and *R*) reported tinniti; and one (*C*) did not. We, therefore, took the precaution of selecting stimulus-tones that did not correspond in pitch to the tinniti reported. As the experiment proceeded, however, we came to realize that there was a continual shifting in the strength, insistency, and pitch of these subjective sounds. This was particularly true in *N*'s case. Shortly after beginning the liminal determinations, *N* reported a steady tinnitus that was approximately the pitch of Tone 3. Liminal determinations for this tone subsequently became extremely variable and unreliable. Later other tinniti developed for this *O* which were close to the pitch of the other stimulus-tones. The experiments with *N* had consequently frequently to be omitted, and sometimes a series had to be stopped in its course because of the difficulty that this *O* experienced in distinguishing the

TABLE III

SHOWING FOR EVERY *O* AND FOR ALL OF THEM TOGETHER THE PERCENTAGE OF TIME THAT ALL THE TONES AND EACH OF THEM SEPARATELY WERE HEARD DURING THE FIRST, SECOND, AND THIRD MINUTE OF THE 3-MIN. PERIOD OF OBSERVATION

| *O* | Tone | Minutes Observed | 1 min. | 2 min. | 3 min. |
|---|---|---|---|---|---|
| C* | All | 57 | 46 | 44 | 42 |
| *Er* | All | 126 | 52 | 55 | 51 |
| *N* | All | 96 | 56 | 60 | 64 |
| *R*† | All | 114 | 47 | 43 | 48 |
| *Sh* | All | 141 | 58 | 54 | 55 |
| All | 1 | 123 | 47 | 47 | 46 |
| All | 2 | 288 | 55 | 54 | 56 |
| All | 3 | 123 | 53 | 53 | 53 |
| All | All | 534 | 53 | 52 | 52 |

* With second attitude; † After recovery from infection.

objective tones from the subjective. Control observations (*i.e.* experiments in which no tone was presented) were also run with *N* to determine whether he could distinguish between the objective tone and his tinniti. If he reported a tone under these conditions, the experiments were either postponed, or in case they had already been begun, the previous reports of the day were discarded as being unreliable. As the experiments progressed, however, *N* became accustomed to his tinniti and felt that he was able to use them as a background in determining the presence of the tone. He was not, however, always successful in doing this. Excerpts from two of his protocols illustrate the effects of his tinniti.

(Tone 3, R ear) Very faint, clear fluctuating tone in right ear. Extremely loud tinnitus in the same ear obscured tone at first, and it was isolated with some difficulty.

(Tone 2, L ear) Tone in left ear—lower in pitch than tinnitus in right. Responded at once and released key whenever dominant pitch of complex took an upward leap—depressed when dominant pitch returned to original level. In other words, used tinnitus as background and tone as foreground—until relations were reversed or when *figure* disappeared.

*R* had pronounced and disturbing tinniti during the time of his aural infection. In fact, observations could not be run for some time because of these pronounced disturbances. Before and after this infection he had a few, relatively insignificant tinniti.

(Tone 2, R ear) Tinnitus is still present but does not seem to interfere as much as it did the last time I observed. They (tinniti) are all much higher or lower than this tone.

(Tone 2, L ear) The tone remained constant and the tinnitus fluctuated. I have developed a new one—very low—which fluctuated markedly and sometimes almost masked the tone.

(Tone 3, L ear) I heard at the beginning of the period something which I took for the tone in the left ear. But now some more tinniti have come in in that ear, and I'm not sure that what I heard was not a variation of one of them.

*C* never reported tinnitus in his protocols. Questioning revealed that tinniti had occurred at most two or three times during the course of the experiments. On these few occasions the tinniti were weak and occurred toward the end of the observational period.

*Er* suffered some from the effects of tinnitus, but control runs showed that she was able to differentiate between the objective and subjective tones.

(Tone 3, L ear) I have a tinnitus slightly higher in pitch than the tone and there is a constant struggle to hear only the tone given, to analyze it out of the beats it makes with the tinnitus. Most of the time this is possible and then the tinnitus fades.

(Tone 2, L ear) It was an awful strain to keep it. The tone made beats with the tinnitus. I find it hard not to let that determine whether or not the tone is present.

*Sh* was also bothered somewhat by tinniti, occasionally having some difficulty in distinguishing them from the objective tone.

(Tone 2, R ear) During the last few seconds I heard a tone that was very high in pitch, localized in the front of my head just above my nose. It didn't disturb much for it was so much higher than the tone you were giving me.

(Tone 2, L ear) Almost at the first of the observation several high tones came in and drowned out the tone I was listening for. They kept on but several times I was able to hear the tone I was listening for along with the other tones.

In general the tinnitus was distracting and made observing more difficult. In some instances it was so like the objective tone that the two were differentiated with difficulty—if at all. Polygraph records, made when the tone was cut off unaware to *O*, indicate that the tinniti sometimes fluctuated in the same way as did weak tones. At other times, however, they were steady and reported as continuously present. In this case *O* sometimes mistook the tinnitus for the tone, particularly when the pitch and intensity of the tinnitus and the tone were similar. When the tinniti were rough, large, and intense, masking of the tone often occurred. *Er* occasionally reported beats between tinniti and the objective tones, and on these occasions she used the beats as an indication of the presence of the objective tone. When the tinniti were weak and not of the pitch of the objective tone, they often served as a part of the tonal field (background), and furnished a setting for the tone (figure). This is the only other instance where tinniti aided *O*. Our results indicate that tinniti are very common, and have a pronounced—usually disturbing—effect on the perception of liminal tones.

*(6) The performance of the observer.* Thus far we have satisfied the first two points of our study (1) by showing that both pure and complex tones fluctuate, and (2) by showing the relationship of the fluctuations to various factors of the experimental situation. Our third task (3) is to give an account of the performance of the *O* during the observational period.

At the beginning of each experimental period the stage is set by the formal instruction for *O* to listen for the tone, to press the key when he hears it, and to release the key when it disappears. During the early or first part of the experimentation, formal instruction plays an important rôle—being supplemented by self-instruction. As the experimentation proceeds, however, formal instruction plays a less important rôle. Predispositions, sets or neural trends develop, and serve as the general background for self and occasional instructions, which specifically set the task. The beginning of every 3-min. period finds, then, the task to report determined in this way.

The observing has, however, only begun. While observing, the individual performs in a variety of ways, utilizing many resources to satisfy the task—often a difficult one because the limits of performance are approached. Our protocols indicate that *O* satisfies the task generally in two different ways: (a) by perceiving, and (b) by searching and inspecting.

Common to the beginning of every 3-min. period is search initiated by the task. When the tone is comparatively loud and clearly audible in a relatively stable and uniform auditory field, search is very short-lived, and the task of hearing the tone is resolved primarily by perceiving. A few excerpts from protocols will illustrate this.

(*C*, Tone 2, R ear) There was little change of any sort. Weak but clear.

(*Er*, Tone 2, B ears) Quite definitely there, though sounded as if it were muffled by a blanket. Pitch definite, fluctuations in intensity.

(*N*, Tone 1, B ears) Low, clear continuous tone in right ear—a little ambiguous in localization. Continued without change. Decrease in determination of task, with necessity for frequent self-instruction to return to job.

(*R*, Tone 2, R ear) Tone easily heard throughout. It fluctuated slightly in intensity, pitch and fuzziness of pitch but not to a great extent.

(*Sb*, Tone 2, B ears) Very clear and distinct high tone localized in the top of the head.

Ordinarily the presence of tinnitus complicated the task, but occasionally *O* apprehended a relatively strong and steady tinnitus as the tone. In this case perception was again adequate for the task. An excerpt from a protocol shows this.

(*N*, Tone 1, R ear) Low obscure tone in right ear. Appeared to be vibratory in character, but the direction and type of change cannot be described. No fluctuation or suggestion of it.

These simple conditions, however, were rarely fulfilled under the conditions of our experiment—due to a variety of factors. In the first place, the intensity of the tones was usually so low that some or most of the attributes usually ascribed to a tone were not present. A more important factor was the presence of numerous tinniti in the auditory field. Under

these conditions the tone often lost its tonal characteristics or disappeared in the tonal field. Here we find that a more lasting search occurred and that *O* was continually inspecting various tonal objects in order to find the tone, *i.e.* to satisfy the task. In the carrying out of the task by means of search and inspection, self and occasional instruction played an important part. Some parts of protocols will indicate the nature of this way of performing, showing specifically the part played by search.

(*C*, Tone 1, R ear) This sound was little more than volume. At the crests of the fluctuations pitch, though very obscure, was present. The change was largely in concentration and with it the contours began. By contour I simply mean figure and ground differentiation.

(C, Tone 1, L ear) Near the end I lost the sound and could not find it. Usually in such a case the tone will reappear at approximately its original strength. This one did not.

(*Er*, Tone 2, R ear) Very thin, faint tone at the beginning. Became less and less like a tone with the course of the observation. Practically lost all pitch and intensity until I wasn't sure whether it was there or not.

(*Er*, Tone 3, B ears) As a rule I observe with my eyes closed. All of a sudden there is nothing but emptiness in the room. The earphones feel as black and as empty as the room looks. I open my eyes and get tense, listening for it.

(*N*, Tone 1, R ear) Lost it entirely about half way through observation and never regained it with any degree of confidence.

(*N*, Tone 3, L ear) Toward the end heard scratching noise again—self-instruction to look for tone—none there—no report.

(*R*, Tone 2, B ears) This time it was more difficult because the tone was more diffuse—poorly localized.

(*R*, Tone 2, L ear) Except for one brief faint sound I could not find anything like the pitch. I recognize now, however, that I had a definite instruction to search toward the left for the sound because the one brief period I had heard it and the preliminary tone had been farther toward the left than the last one had been.

Here we have evidence of search as initiated primarily by occasional instruction, *i.e.* by the absence or the disappearance of a tone, or by a loss of its tonal characteristics. There is also some indication of the beginnings of inspection. The resolution of the task by search and inspection is more clearly shown, however, in the following protocols.

(*C*, Tone 2, L ear) I did not report immediately because I was not sure that the imaginal context was not supplying the tone. Inspection clearly showed this not to be an imaginal accompaniment.

(*C*, Tone 2, L ear) There are times when perception of a very passive sort is sufficient to determine the presence of the tone, but as the tone approaches the limen perception must be changed to inspection. It seems impossible to inspect the tone throughout long periods. Since a negative report must also be determined by inspection there is a portion of the time in which either a positive or negative report is not accurate. Often I find that if inspection be confined to short but frequent intervals a more reliable report can be made.

(*Er*, Tone 2, B ears) It was like a visual afterimage which you try to inspect more closely and it floats off toward the periphery and disappears. I found myself doing just that—looking straight ahead and then, as the tone got fainter, looking further and further toward the right.

(*Er*, Tone 3, L ear) I have a tinnitus slightly higher in pitch than the tone and there is a constant struggle to hear only the tone given—analyze it out of the beats which it makes with the tinnitus.

(*N*, Tone 1, B ears) Tone lower than tinnitus heard. Cues used: an occasional drop in pitch of tinnitus which seemed to indicate presence of tone. Never certain of presence, but seemed much lower and fainter than tinnitus. Constant self-instruction to avoid interpreting tinnitus since latter appeared to fluctuate a good deal.

(*N*, Tone 2, B ears) Finally successful in weeding out a lower tone from a complex and responding to it. Never certain—tone poorly localized and vague in character.

(*R*, Tone 2, L ear) I am by no means sure that I heard the tone at all. After much searching I discovered something localized in back of my head which had vaguely the pitch of the previous tone, but which was much more diffuse. It had had more the character of a tinnitus.

(*R*, Tone 3, B ears) I had some difficulty in finding it because of the confusing mass of other tones. But when I did succeed in singling it out there was no doubt that it was the same tone I had listened to before.

(*Sb*, Tone 1, L ear) A very faint tone that filled my left ear. It would fade and I tried various means of finding it. I turned my eyes in that direction, I checked my breathing and tensed the muscles on that side. Sometimes I could bring it back by that method but never for more than a brief period.

(*Sb*, Tone 2, R ear) During the whole period I was somewhat strained toward the right. I leaned that way, my eyes were turned to the right, and the muscles strained on that side. This was intensified when the tone seemed to be leaving.

From these results we conclude that the task of *O* as set by formal, self and occasional instruction is satisfied in two ways. The first is a perceptual performance, occurring when the task is relatively easy; the second way is by means of search and inspection when the task is more difficult and cannot be resolved by perceiving.[28]

## Conclusions

We have attempted to place the problem of 'fluctuation' of liminal tones in its proper historical setting; to show the dependence of 'fluctuations' upon stimulus-intensity and wave-form; to show the range of intensities giving fluctuations; to show the day-to-day and short-period changes in auditory acuity; to discover evidence of auditory adaptation; to demonstrate the effects of tinnitus upon 'fluctuations' and upon *O*'s performance; and

[28] For an account of the various modes of instruction and for the differentiation between perceiving and inspecting cf. Madison Bentley, *The New Field of Psychology*, 1934, 438; and J. McV. Hunt, A functional study of observation, this Journal, 47, 1935, 1-39.

to give an account of what *O* is doing during the experiment. We feel justified in drawing the following conclusions from the results of our study.

*(1)* Perceiving of pure and complex tones of liminal intensity is an intermittent performance.

*(2)* The frequency of this intermittency is primarily a function of stimulus-value, the most rapid 'fluctuations' occurring with tones perceived approximately half of the time.

*(3)* The range of intensities giving fluctuations is very narrow, often less than the day-to-day variations of auditory acuity.

*(4)* Adaptation to liminal tones—as measured by auditory acuity during a 3-min. period—does not occur.

*(5)* Tinniti or subjective tones greatly complicate the performance of of the observer, and also affect the 'fluctuations.'

*(6)* Perceiving is adequate for the apprehension of a weak tone under relatively simple conditions; but a decrease of intensity and complicating of the auditory field by tinniti usually lead to searching and inspecting.

*(7)* 'Fluctuations' are due to a complex relationship between the adequacy of the effect of the presented tone and the other excitatory factors of the peripheral and central apparatus for hearing—some of which are observed as tinniti. The operation of these factors seemingly accords with the phi-gamma hypothesis.

# NAIVE GEOMETRY IN THE PSYCHOLOGY OF ART

By R. M. OGDEN, Cornell University

What lends beauty to a scene? An answer to this question can be found in a geometrically systematic order which will account for the appearance of certain things and events of beauty. It may be assumed that things seen have geometrical implications, because every visual perception is an arrangement of spatially disposed parts and members.

The main conditions for seeing anything are determined by the behavior both of the eyes and of the body. A study of these conditions carries us at once into the physiology of vision, including color-sensitivity, eye-movement, and posture. We do not perceive empirically either the physiological processes of the retina and the nervous system, or the muscular adjustments of lens, iris, and eyeball. Even of our posture we are but vaguely aware. Yet inferences concerning the probable nature of all these processes and adjustments supply an understanding of visual space-perception.

When and under what further systematic conditions will an object seen be invested with beauty? The answer to this question is primarily geometrical, though secondarily it will imply many other conditions which we shall here neglect. We can afford to neglect them, not because they are unimportant in rendering a perception beautiful, but because, without distinguishing a figure from its ground, nothing at all is perceived. When a spatial relationship is once established between a figure and its ground, the figure can mean this or that, and it can be enriched by this or that sensory feature, by the memories and fancies it evokes, and by the behavior in which it engages us. Indeed, its beauty may rest primarily upon one or more of these effects. What concerns us here, however, is the beauty that arises from geometrical conditions alone. These we may regard as the conditions of sheer visual beauty, because all other conditions are secondary and derivative. Under inappropriate spatial conditions the beauty of a scene is, at best, ephemeral. Only a geometrical system, inferred from the empiricial data of vision, will account for the more and less favored views of things seen.

We are prone to think of geometrical conditions as the postulates of a

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geometer. It appears to be still possible for mathematicians to regard axioms as self-evident intuitions, without reference to their genetic antecedents in behavior. However this may be from the point of view chosen by a mathematician, the psychologist cannot be content to accept any axiom, such as the statement that a straight line is the shortest distance between two points, without wishing to trace its genesis in the behavior that has given rise to the empirical phenomena of straightness and relative shortness. It is with phenomena like these that we are concerned when we talk in terms of *naïve geometry*. We mean, in a word, those favored forms which, in perception, are outstanding, and, if you please, self-evident, because they are conditional upon a geometrical system.

For instance, a point is a perceived object which lacks dimensions of length, breadth, and thickness. A line, seen or touched, moved over by eye or finger, has extension but no breadth or thickness. A surface has areal extension but no depth, and a solid is an object which has both an exterior and an interior, over which and through which movement can be made. The meanings of these geometrical features of perception are all self-evident aspects of behavior.

Psychologically considered, self-evidence attaches to any behavior that records itself as a complete and unanalyzable thing or event. Self-evident things and events are apprehended as wholes which when compared with other wholes suggest analysis; but these wholes are the primary data of analysis, and not its products.

What then are the self-evident wholes of vision? They are, to begin with, points, lines, surfaces, and solids. Points are univocal; lines may be straight or curved; surfaces and solids have shapes, the contours of which are seen as straight or curved lines. There are, to be sure, shapes the contours of which are indefinite, as when we see an object through a haze which softens and sometimes destroys the precise outline. Under these conditions an object loses its sharp edges, and its corners are blurred and may disappear altogether.

All self-evident shapes, however, fall in fairly distinct classes: those bounded by curved lines, such as *circles, ellipses,* and *ovoids,* and those bounded by straight lines, such as *triangles, rectangles,* and *polygons*. These, when discovered are outstanding and self-evident in comparison with all variant forms of less regular contours. Thus a child, who usually sees rectangular objects like table-tops in perspective, will discover the true shape of the top only after he has seen or felt it as a rectangle. Once made, a discovery of this kind is and remains decisive. The one rectangular shape is favored, and becomes the essential meaning of the table to which all

variant perceptions of it are afterwards referred, as to a norm. So strong is this influence that in drawing a rectangular object seen in perspective both the child and the artist underestimate obtuse and overestimate acute angles, thus bringing them nearer the right angles which they are.

We learn to perceive things, each in terms of the self-evident, integral, pattern which a fortunate discovery reveals to be its prototype. The apprehension of these prototypes is primarily esthetic. Without rationalization, or any enumeration of parts, the prototype stands out self-evident in its unique quality of fitness. Its artistic quality may, indeed, be slight; for it is only the beginning of a formal pattern which, if developed, might engage us fully and give complete satisfaction to our need of expression.

Yet these simple, geometrically self-evident, forms are the basis of design, just as they are the basis of geometrical science. The difference between an esthetic and a scientific pursuit of geometry is the difference between naïve and rational modes of expression. The elaboration of geometrical forms in painting and sculpture, in decoration and architecture, is guided by the way these forms feel as they are seen by the roving eye, and explored by the moving hand. In scientific geometry the same favored forms furnish a point of departure for reasoned inferences.

Naïve and reasoned geometry meet in planning a building wherein the geometrical forms are reduced to a linear scale which the builder follows as his specifications. But it is the naïve and not the measured units that lend artistic effect to the structure and thus invest it with beauty. No architectural plan or elevation which gives the specifications of a building is thereby a beautiful expression. Neither is any cartoon for a painting on which are marked the proportional dimensions of the design.

From the history of art it would appear that whenever naïve geometry has been discarded, or overlaid with rational geometry, the result has been a marked deterioration of artistic design. Such a deterioration is evident when we compare the temples of ancient Greece with their Roman counterparts. During the four or five centuries between the Periclean period of Greece and the Augustan period of Rome, the canons of geometrical design were lost. Vitruvius, writing shortly before the Christian era, has much to say of the fine symmetry and proportions of the classic order; but he obviously had no clear understanding of the geometry of this order. The "modules," to which he frequently refers, are seemingly arbitrary divisions. When he describes the Doric order in the hexastyle temple, he divides the width of the stylobate in forty-two parts, allowing two parts, or modules, for the diameter of each column at its base, and fourteen parts for the height of

each column, including its capital. Hence, the column-façade below the architrave becomes an oblong in the proportion of one in height to three in width.[1]

Simple arithmetical proportions, like the one given, do not appear in the Doric temples of the sixth and fifth centuries B.C. Instead, their proportions are those of geometrical progressions which cannot be reduced to arithmetical units.

The ancient Greek modules of design seem to have been derived from geometrical figures in which the over-all dimensions are not given as so many units of linear measurement, but as the interrelated forms of squares, "root rectangles," and the derivative extreme and mean ratio of the "golden sector."

Since the dimensions of many of these forms are incommensurable, they cannot be constructed by means of a linear scale. With the aid of a compass, however, or a

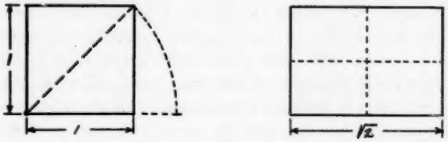


FIG. 1. SQUARE AND ROOT-TWO RECTANGLE

peg and string, to serve a similar purpose, these figures become the natural results of geometrical planning. Root-rectangles and the golden-sector proportion are also related to the square in the following manner:

*Square.* The diagonal of a square (Fig. 1) is incommensurable; since the sides of the square are taken as units, the square of the hypotenuse, or diagonal, will be 2, and its length the square-root of 2. A rectangle with this length as base and with

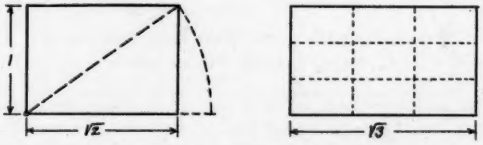


FIG. 2. ROOT-TWO AND ROOT-THREE RECTANGLES

unit-sides is called a root-two rectangle. Its base is the irrational number 1.4142+. This figure is divisible in two parts, each of the same shape, or proportion, though in upright position. These two figures are in turn divisible in two smaller figures of the same shape in horizontal position, and so on, indefinitely.

*Root two rectangle.* The diagonal of a root-two rectangle (Fig. 2) will have the

[1] Frank Granger, Vitruvius on architecture, *Loeb Classical Library*, 1, 1931, (Bk. IV), 221.

incommensurable length $\sqrt{3}$, or 1.732+. With $\sqrt{3}$ as base and with unit-sides, the root-three rectangle is formed, which divides itself in three vertical rectangles of the same shape, each of which is again divisible in three, and so on indefinitely.

*Root-three rectangle.* The diagonal of a root-three rectangle (Fig. 3) measures the square root of four, or 2. With this length as base, we have the root-four rectangle, composed of four upright rectangles, each in turn divisible in four rectangles, of the same shape. But this figure is also divisible into squares—two, eight, thirty-two, and so on. It has therefore certain arithmetical and enumerative, as well as certain geometrical properties.

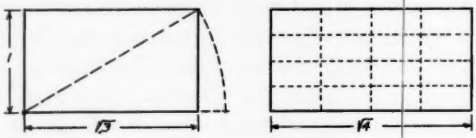


FIG. 3. ROOT-THREE AND ROOT-FOUR RECTANGLES

*Root-four rectangle.* The diagonal of the root-four rectangle (Fig. 4) will measure the square-root of five, or 2.236+. The rectangle with this length as base is divisible in five vertical rectangles of the same shape, each of which is in turn divisible in five horizontal rectangles of like shape, and so on. But this figure has yet another characteristic, for it includes two rectangles of the divine or golden-sector proportion, one placed horizontally and the other vertically to complete the figure.

This golden-sector proportion, or the "extreme and mean ratio" is the most significant of all the geometrical proportions. As a linear division of a line in two parts, the golden sector is that division in which the parts are most highly in-

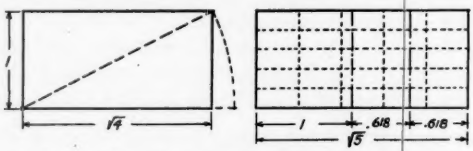


FIG. 4. ROOT-FOUR AND ROOT-FIVE RECTANGLES
(Showing included Golden Sectors)

dividuated. The impression made is one of uniqueness. When the shorter division is to the longer, as the longer is to the sum of the two—a:b::b:a+b—there is no common unit to which both terms are referable. Instead, we have here a classical instance of William of Occam's razor: *Entia non sunt multiplicanda.*

The linear dimension of this ratio is the incommensurable number 1.618+ and the proportion, when extended, is expressed by the series of numbers: 0.09+, 0.146+, 0.236+, 0.382+, 0.618+, 1, 1.618+, 2.618+, 4.236+, and so on.

It will be noted that each term of this series is the sum of the two preceding terms. With whole or commensurable numbers, a similar series, known as the Fibonacci or Lamé series, is the following: 1, 1, 2, 3, 5, 8, 13, 21, 34. . . . This series of whole numbers constantly approaches, though it never quite attains, the extreme

and mean ratio. But in producing this ratio by linear means, the proportion 5 to 8 (1: 1.6) is a close approximation.

We see, then, that the root-proportions generated from the square on its diagonal

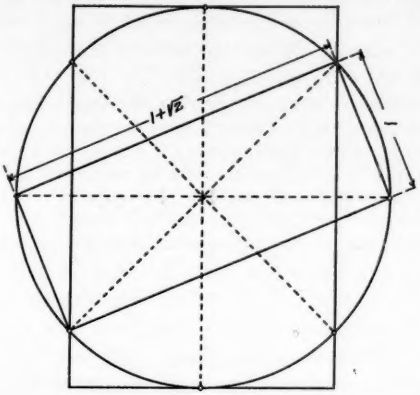


FIG. 5. CIRCLE ANALYSIS BY FOUR AND EIGHT EQUAL DIVISIONS OF THE CIRCUMFERENCE

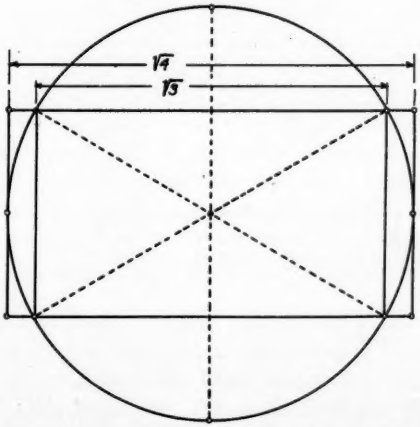


FIG. 6. CIRCLE ANALYSIS BY SIX EQUAL DIVISIONS OF THE CIRCUMFERENCE

give rise to a series of related forms, and when we reach the root-five rectangle we have one which also includes the unique ratio of the golden sector.

These related proportions furnished a basis for the design and construction of many antique monuments, both Egyptian and Greek. Vitruvius' explanation of the plan of a temple in terms of "modules" which, when added together, give the dimensions

of the building are not in accordance with the measurements of the early Greek and Egyptian monuments. These seem rather to have been designed with the aid of circle-analyses which give rise to root-rectangles and golden-sector proportions.

*Circle.* When the circumference of a circle is divided in eight equal parts (Fig. 5), the tangent of two parts will form a root-two rectangle; that is, one the proportion of which is $1:\sqrt{2}$. Similarly, an inscribed rectangle formed with a chord of the eighth sector will have the proportion $1: 1+\sqrt{2}$.

A root-three rectangle is obtained by dividing the circumference of a circle in six parts (Fig. 6), and drawing the enclosed figure between two opposite chords. If this same rectangle is extended to the tangents, it will become a root-four rectangle.

Divisions of the circle in five and ten parts are even more interesting (Fig. 7), because they introduce a number of golden-sector proportions.

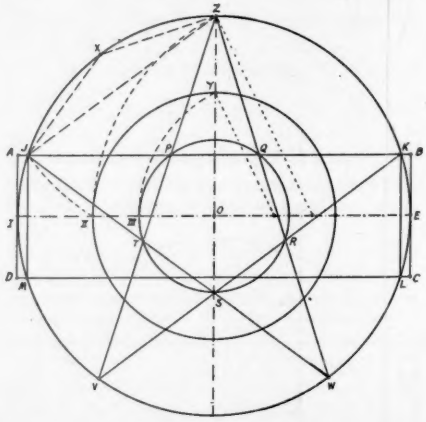


FIG. 7. CIRCLE ANALYSIS BY FIVE AND TEN EQUAL DIVISIONS OF THE CIRCUMFERENCE
The manner in which Golden Sectors are obtained is also indicated. Note that Z-II equals Z-J, one side of an enclosed pentagon, and O-II, the Golden Sector of Radius O-I, equals X-J, one side of an enclosed decagon.

The conclusion of the German architect, Ernst Mössel,[2] is that the ancient Greeks and Egyptians drew their designs with the aid of a compass, or a peg and string. Thus they found their proportions by such means as have been briefly described. As a result, the over-all plans, elevations, and subordinate members of their buildings fit into geometrical progressions, and rarely permit of reduction to linear units.

With the development of linear means of measurement, these proportions were approximated to unit-lengths, or modules, in the sense in which

[2] *Die Proportion in Antike und Mittelalter,* 1926, 1-128; *Urformen des Raumes als Grundlagen der Formgestaltung,* 1931, 1-202.

Vitruvius writes. The resulting structures lost the integrity of wholes, the members of which were geometrically disposed parts, and became aggregations of some arbitrary unit. At first, perhaps, it was the diameter of a column; but later the more abstract length of a foot, or some other arbitrary unit of measurement, came into use.

The architect, however, still had his eye and his compass, and certain rule-of-thumb methods of design and construction. Therefore, empirical relics of the older canons of craft in proportional and symmetrical design lingered on through the Roman, Medieval, and Renaissance periods.

Our interest here lies in distinguishing the psychological effects of two principles of design, which may be called proportional and enumerative; these being the two chief ways in which the underlying geometrical system of a figure-ground pattern is perceived. While proportional behavior is concerned with progressions, enumerative behavior is concerned with the recognition of units and their elaboration by addition. Naïve geometry suggests the immediate apprehension of symmetries which develop and grow in the manner suggested by the root-proportions and the golden-sector divisions. Designs that reveal symmetries of this order are called *dynamic* and, as we have seen, they are attained with the aid of tools like the compass, and require no linear scale of measurement. Naïve geometry also suggests the immediate apprehension of another type of symmetry based upon the use of a "module" which is a natural unit of the design. The repetition of this module by the arithmetical processes of addition, subtraction, multiplication, and division, gives rise to a symmetry called *static,* because it does not grow from form to form by progression, but merely repeats itself in larger and smaller duplications of the same shape. These two principles of naïve apprehension lead respectively to the systems of plane and solid geometry, and to those of enumeration, first in terms of concrete, and then in terms of abstract units.

In maintaining that artistic design is a product of naïve geometry, rather than of reasoned inferences, whether geometrical or arithmetical, emphasis is placed upon the direct apprehension of natural forms which give immediate satisfaction by virtue of their self-evidence. A design which is conceived as a direct expression of figure-ground space relations will reveal the underlying principles of static or dynamic symmetry, and sometimes, as is the case with the root-four rectangle, it will embody both principles.

Such a design is necessarily harmonious because its natural parts or members are derived from, and therefore belong to, the whole of which they are parts. When the symmetry is "dynamic," every part has a functional relationship, both to every other part and to the whole of which it is a part. On the basis of a geometrically

proportioned design, modifications and variations will be introduced to meet practical needs, to correct illusions, and to soften or sharpen contrast-effects, wherever these appear to be required. For instance, the space between the middle columns of a temple may be widened to permit, as Vitruvius says, "a convenient approach."[3] The angle-columns, too, are sometimes made thicker "because," writes Vitruvius, "they are cut into the air and appear more slender to the spectators."[4] The *entasis* of the columns, and the curvature of steps, stylobate, eaves, and even the architrave, have the effect of softening what might otherwise appear as rigid and sharply-edged lines. They also correct optical illusions which sometimes make straight lines appear as if they were curved.

Enumerative design differs from proportional design in that the composition begins with a convenient unit of measure, into which the whole space may be divided. One can either start with the whole, and divide it into a convenient number of units, or start with the unit and multiply it until a desired whole is obtained. In both cases the unit tends to be an integer rather than an integral of the whole, and lines of cleavage may be felt between the unit-parts. In any event, the result is not a dynamic growth but a sum or accretion of parts. Accordingly, a design of this order is called *static*. Though not without its own kind of beauty, the psychological effect of static symmetry is one of stillness and placidity. Its members have less functional unity than do those of a dynamic geometrical design in which there are no units that subdivide the whole, but only those that are true members of the whole.[5]

To be sure, the perception of any design is an act of behavior. Whether static or dynamic, it must suggest modifications and variations which tend to hold it together. Neither a geometrical nor an arithmetical principle can be followed slavishly and result in a work of art. Therefore, contrasts within its pattern, and with its environment, will often be softened or sharpened; illusions will be corrected, and practical needs met, by modifying the systematic order in accordance with which the design as a whole is composed.

The beauty attaching to either dynamic or static forms of composition rests upon a harmonious perception; but what we call a structure of static symmetry is calculated in terms of its equal units, their addition, subtraction, multiplication, and division. A structure of dynamic symmetry, on the other hand, is calculated in terms of a geometrical progression of shapes which repeat themselves in varying positions, with variable dimensions that are irreducible to any given unit of measurement.

These principles of dynamic and static symmetry find a parallel in music. The harmonic music of the so-called diatonic mode is derived from ancient Greek usage. The intervals and chords of this scale are afforded by divisions of the octave which

[3] Granger, *op. cit.*, 173.
[4] *Ibid.*, 179.
[5] Cf. Jay Hambidge. *Dynamic Symmetry: The Greek Vase*, 1920, 161. Chapter XII deals briefly with static symmetry.

give unique, recognizable, units, or modules, known as the fifth, fourth, thirds, sixths, whole-, and semitones. These are dynamically interrelated as a series of progressions within the octave. The particular intervals mentioned possess a unique character akin to those of the square, the root-rectangles, and the golden-sector proportion.

In contrast, some Oriental music, notably of Siam and Java, is based on equal intervals and their addition and subtraction. The musical pattern that results is a simple melodic invention. The unit-interval, or module, has only an arithmetical relation to other unit-intervals and their multiples. There is neither harmony of chords nor counterpoint, as we use it, and there is no subtlety of combination such as results when two whole-tones and a semitone give rise to a new and self-evident unit, the musical fourth.

The compromise which has been effected in modern Occidental music by the developed employment of a musical scale of "equal temperament," in which the diatonic mode has been reduced to the compass of a chromatic scale of twelve equal semitone intervals, illustrates the possibility of achieving a musical order of static units with dynamic possibilities. Similarly, the reduction of the dynamic modules of ancient architecture to the commensurable units of a scale of linear feet which can be arithmetically calculated as the specifications of a building, illustrates the manner in which the sensibility of the eye for dynamic effects has been reduced to rule-of-thumb methods under arithmetical guidance.

The development of human ways of apprehension from naïve sensibility to precise calculation involves a psychological change of deep and wide-spreading significance. While the meaning of things seen resides in favored forms, precise calculations leads to abstractions which often violate the naïve unity of a perceived whole. We are confronted with a paradox. Our procedure in seeking explanations has been to define and enumerate entities that have no real existence, because *entia non sunt multiplicanda.* In fact, we find ourselves rationalizing what is essentially irrational, and building things of parts that have no functional relationship; for the ordered system of nature, consisting of real units and progressions, we substitute a man-made order in which the accretion of artificial units is made to account for things which in reality possess no corresponding units.

Art alone resists this device of reducing all things to abstract units. By recourse to abstract rules of order we can make a sentence, build a house, paint a picture, or compose a melody; but beauty fails to appear in the product unless the parts fuse to form a recognizable whole. Thus static symmetry appeals to a simple method of comprehension that gives satisfaction by virtue of our ability to see the units which go into the composition of the whole design. Unless these units can readily be recognized, the design of the whole is lost to view. Likewise, a dynamic symmetry that is over-subtle in its dependence upon geometrical progressions which cannot be directly seen, loses its artistic significance.

Masterpieces of classical design, both Greek and Egyptian, reveal geometrical proportions of the most intriguing order.

Let us take the Athenian Parthenon as an example. Perhaps the most atypical of classical structures, the Parthenon reveals geometrical calculations of a highly erudite character. Its geometrical progressions, however, are mainly visible in outstanding features of the temple. Many analyses have been made of this remarkable structure, with divergent and sometimes conflicting results. Yet the fact remains that the building possesses geometrical features of such refined accuracy that a mere coincidence of fact with theory is out of the question.

In illustrating this point we shall confine ourselves to the temple's leveling course, steps, and stylobate, or platform.

The *Euthynteria,* or leveling course, is partly rough-hewn from the rock, and serves as foundation for the present temple, as it did for an earlier hexastyle temple destroyed by the Persians in 480 B.C. This leveling course measures 111.31 by 238.003

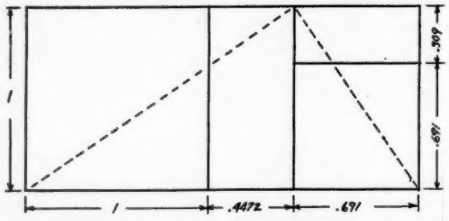


FIG. 8. GEOMETRICAL ANALYSIS OF THE EUTHYNTERIA

ft. The proportion, 1:2.138+, is itself an interesting geometrical figure. As Hambidge has pointed out,[6] it contains (Fig. 8) a square of the shorter dimension, a root-five rectangle with length of this dimension, while the remainder is made up of the same two figures; namely, a smaller square and a smaller root-five rectangle. Further, a diagonal from the lower left-hand corner of the larger square to the upper right-hand corner of the larger root-five rectangle forms a right-angle with the diagonal drawn from this point to the lower right-hand corner of the whole figure. What importance may attach to this angle, except as a geometrical means of constructing a right angle, is not evident.

According to Hambidge's analysis, squares (or cubes) and root-five rectangles (or parallelopipeds) constitute the over-all structure of the building as a whole. But none of the precise linear measurements of the figures contained in the Euthynteria recur in the plan or superstructure of the building.

Richter has shown how the proportions of the building (see Fig. 9) are related to those of the stylobate and to the bottom step of the temple.[7] If the length of the bottom step on the side be made the diameter of a circle, and the radius of this circle be marked off in a succession of golden sectors, it will be found that the sum of two of these sectors (II and IV) is exactly the width of the stylobate.

[6] Hambidge, *The Parthenon,* 1924, 10.
[7] Irma A. Richter, *Rhythmic Form in Art,* 1932, 1-127.

Numerically, this decreasing progression, which has already been given, is 1, 0.618+, 0.382+, 0.236+. The length of the bottom step measures 237.44 ft. The radius of the circle is then one-half this amount, or 118.72 ft. Sectors II plus IV equal 0.854+ which, when multiplied by 118.72, equals 101.38+. The measured width of the stylobate is 101.36+ ft.

On the basis of the length of the lower step, we have obtained the width of the top step, or stylobate. The corner of the stylobate is obtained with the aid of a second circle which encloses Circle I, and has the same center. This larger circle is so constructed that the diagonal of its enclosed pentagon equals the basic length of the bottom step. Since the proportion of the radius of a circle to this diagonal

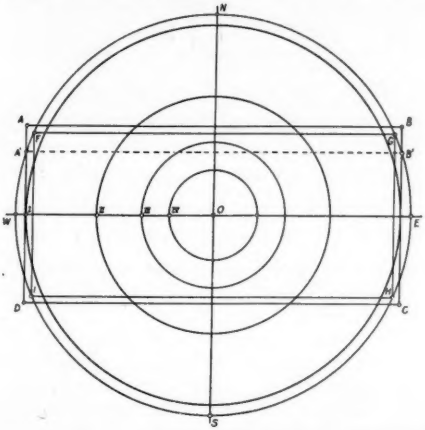


FIG. 9. GEOMETRICAL PLAN OF THE LOWER STEP AND STYLOBATE OF THE ATHENIAN PARTHENON OBTAINED WITHOUT MEASUREMENT BY CIRCLE ANALYSIS IN TERMS OF GOLDEN SECTORS

is 1:1.902,[8] the radius of the large circle will be 124.836+ ft. Forming as it does the hypotenuse of a right-angled triangle, it is possible to compute the square of this number, then deduct from it the square of half the width of the stylobate (50.69), which is the smaller side of the triangle. The square root of the difference, 13014.55+, equals 114.08+. The actual measurement of the length of the stylobate is 228.14 ft., half of which number is 114.07.

The length of the front lower step appears to be related to the width of the stylobate, being this dimension plus an increment equal to its sixth golden sector. The numerical expression of this sector is 0.09+ (cf. p. 202), and the increment, 9.12 ft., when added to the width of the stylobate, 101.36+, yields 110.48+ ft. The measured length of the lower step is 110.6 ft.

[8] If the radius of a circle is 1, the length of one side of its enclosed pentagon is $[(5+5^{1/2})/2]^{1/2}$, which approximates 1.902.

The construction of this outer circle from the diameter of Circle I is possible in accordance with Euclidean principles without calculating these proportions.

Richter proceeds to analyze other important features of the temple in terms of what she calls a "major scale" of proportions derived from the length of the bottom step on its side, and a "minor scale" of proportions derived from the width of the stylobate.

While a reconciliation of the various analyses made by Hambidge, Richter, Mössel, and Dinsmoor[9] awaits the effort of a competent geometer, it is impossible to suppose that they can be reduced to linear measurements of an arithmetical order. The basic orders must be regarded as geometrical progressions of the divine proportion. Confusing though the attempts to solve the problems of construction have been, the fact remains that the dimensions of the Parthenon are not reducible to commensurable units. Some scheme of geometrical proportions must be adopted to account for the uniquely membered parts of this amazing structure. At least, no means has yet been discovered for its reduction to any single module of a given size.

Psychological experiments have often been made on the choice of simple proportions, when the subject is instructed either to draw a satisfying figure, or to select from a number of figures the one most favored. On the whole, these experiments have been ambiguous. The results do not show that one figure is necessarily more favored than another; and some psychologists have concluded that there are no "favored forms."

In this, as in all other psychological investigations, the conditions under which the figure is formed in apprehension are all important. If you give pencil and paper to your subject, and ask him to draw a rectangle of such proportion as he would like to see, even so simple a task involves many uncontrolled variables. The artist has more or less skill in drawing. He is influenced by his skill and his previous experience. What he draws may give emphasis to the four straight lines, or may emphasize the area which these lines bound. The figure of the lines, and the figure of the shape bounded by the lines, are not the same. Both are influenced by the background, which includes the observer's own posture, by the shape of the paper on which the figure is drawn, and by the position on the paper which the figure occupies. The same artist under these varied influences may now draw one figure, and again another. Even in the act of drawing, the result is influenced by the record as it develops under his eyes. The results may be quite different if the medium of expression is chalk or paint, or if the figure is defined by its surface-shape, rather than by its outline.

One important difference in the results is attributable to the point of view which,

[9] W. B. Dinsmoor, How the Parthenon was planned, *Architecture*, 47, 1923, 177 f.; 48, 1924, 241 f. It should be noted that Dinsmoor rejects proportional analysis in favor of a principle of construction based on the size and positions of the temple columns. While recognizing the geometrical relations to which we have referred, he regards them as coincidences. Their explanation, he thinks, "is undoubtedly to be found in the mathematical theorem to the effect that, between any two lengths selected at random, a great number of simple geometrical relations can always be found, merely by the use of sufficient patience and ingenuity." (p. 180). Granted the truth of this theorem, it can hardly explain the fact that these coincidences all agree with *one* system of proportion; namely, that of the golden sector.

wittingly or unwittingly, moves the artist in performing his task. One point of view leads to the construction of a unit-figure, intentionally uninfluenced by its areal surroundings, or by the means of its production. Such a figure is self-contained, and *centripetal*. It begins with its contour and extends inward, but not outward. Another point of view is *centrifugal*. The figure is intended to be a figure-ground pattern. That is to say, it relates itself to its surroundings, and its production has the wider connotation of a total behavior. Centripetal patterns suggest static symmetry and arithmetical inferences, while centrifugal patterns suggest dynamic symmetery and geometrical inferences. The first point of view leads to specific, the second to general, modes of behavior.

It may not be supposed, however, that forms produced under these two points of view can be reduced to the elements of behavior involved in their production. For instance, there appears to be no direct correlation between eye-movements and the

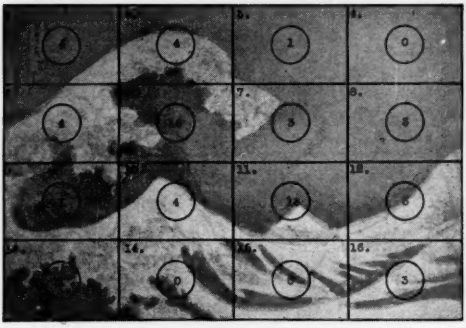

FIG. 10. DISTRIBUTION OF SEVENTY LONG FIXATIONS BY THIRTY-EIGHT SUBJECTS

static or dynamic forms of a perceived design. Buswell has reported a meticulous study of movements made by the eyes in looking at pictures.[10] The eyes tend to fix and rest longest on points and areas of most importance, but the movements themselves are jerky and irregular, and the range of view about the fixation-point is so comprehensive that there appears to be no necessity of following contours or directions, except in a very general way. Though the movements and fixations, individually considered, reveal little of importance, note that in a picture with commanding curves, like Hokusai's *The Wave*,[11] the fixations of Buswell's observers were of longest duration at or about the centers of these curves (see Fig. 10).

Nor may it be supposed that a purely geometrical figure, the proportions of which indicate geometrical progressions of great interest, is thereby made beautiful to behold. Consider the well-known Pythagorean five-pointed star in Fig. 7. This is one of the most interesting of geometrical figures because it repeats the golden-

[10] G. T. Buswell, *How People Look at Pictures*, 1935, 90.

[11] *Op. cit.*, 91 (plate XLVII). Reproduced by permission of the author and the University of Chicago Press.

sector proportion again and again. Yet it is doubtful if this figure can itself arouse a sense of beauty.

On the other hand, its proportions, and the very order of their arrangement, serve well as means of analysis, especially in circular designs. If the essential features of Fig. 7 are superimposed upon Botticelli's famous tondo, known as *The Magnificat,* the results (Fig. 11) are surprising. With the upper point of the star placed at the center of the orb from which rays of golden light illuminate the picture, it will be found that the lines and proportions of the star clarify in many ways the organization of the whole composition. Essential features, such as eyes, nose, mouth, shoulder, elbow, wrist, ankle, middle knuckle, and so on, fall uncannily upon the lines of the star, of the pentagon which encloses its points, upon

FIG. 11. SUPERIMPOSITION OF GOLDEN SECTORS ON BOTTICELLI'S *Magnificat* (Dynamic symmetry)

the axes of the circle, and upon two enclosed circles, the radii of which mark golden-sector divisions of the radius of the complete tondo.

That a correspondence of these numerous features with the geometrical framework is attributable to something more than chance, is demonstrated by comparison with an ancient copy of the same picture which hangs in the Louvre. In the original picture the orb is displaced to the right, thus relieving the composition of over-emphasis upon the fundamental directions, and thereby increasing the effectiveness of the circular composition. In the Louvre picture the orb is at the top. With the same geometrical design of star, pentagon, and golden-sector circles superimposed, it will be found (see Fig. 12) that the number of significant correspondences is less. Not only is the execution of the copy notably inferior to that of the original; the composition, too, lacks subtlety, and the harmony of its total effect in comparison with the original is relatively slight.

That Botticelli laid out his masterpiece on the framework which we have chosen for its analysis, may be doubted, though many painters of his time gave attention

to such matters.[12] But that Botticelli's eye was acutely sensitive to these directions, and to the extreme and mean proportion which these particular directions involve, is too evident to be overlooked.

FIG. 12. SUPERIMPOSITION OF GOLDEN SECTORS ON THE LOUVRE COPY OF THE *Magnificat*

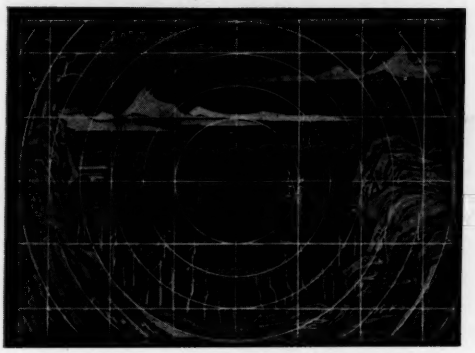

FIG. 13. A CHINESE LACQUER SCREEN
(Static symmetry)

[12] Cf. Fra Luca Pacioli, *Divina Proportione*, 1509. German trans. by C. Winterberg, 1896, 1-367.

In comparison with the dynamic symmetry revealed in Botticelli's *Magnificat*, static symmetry is to be found in many pictures wherein the design seems to spread by waves of equal interval from a center. Two pictures have been chosen to exemplify this type of design. The first (Fig. 13) is a landscape-panel from a Chinese lacquer screen.[33] Though the center of this picture contains no point or figure to catch the eye, it is obviously the focus about which a framework of squares and concentric circles can be placed which is in close agreement with the picture's outstanding features.

The second picture chosen to illustrate static symmetry is the well-known *Lesson in Anatomy* by Rembrandt (see Fig. 14). Here, too, we find that concentric circles of equal interval give place for the nine heads of which the picture is mainly composed. Knowing, as we do from Buswell's investigation,[34] that in looking at pictures

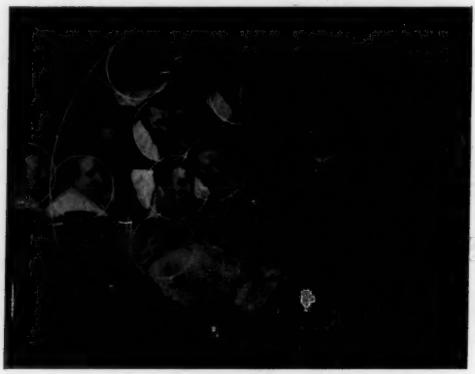

FIG. 14. REMBRANDT'S *Lesson in Anatomy*
(Static symmetry)

our eyes seek and rest longest upon areas of importance, and especially upon the heads of figures, we see in this ordered arrangement of subtle distribution of like units about the center of the picture.

Although the scheme by means of which these two pictures are analyzed is somewhat arbitrary, it nevertheless indicates in both cases a static rather than a dynamic arrangement. It would be difficult to discover a scheme of geometrical progressions which would agree as well with either design.

Naïve geometry is a sensibility to spatial relationship. It can be so fine that the resulting composition will repeat again and again the same proportion as a natural development of the design as a whole. In arriving at the result an artist seems to need no other guidance than a finely sensitive

[33] Cf., E. F. Strange, *Chinese Lacquer*, 1926, Plate LI.
[34] Buswell, *op. cit.*, 90.

eye, associated with a skillful technique. The result is one of symmetry, either static or dynamic, in which all parts of the composition are members of the whole, and occupy their due places and positions as inevitable partial patterns of this whole.

Any great work of design will endure geometrical analysis, not because the artist wittingly employed geometrical means in its production, but because every membered whole must have a geometrical plan of organization. Psychological experiments, in order to reveal the manner in which beauty is produced and apprehended, must have regard for this plan. No study of parts as units will ever reveal the source of integration which accounts for the investment of a design with beauty. Beauty attaches itself to those behaviors and products of behavior which conform to the organizing principles of life under natural conditions of growth and development. These principles account for harmony in action and in perception. They are either static or dynamic in character, and the symmetry of space-relations is attributable to them.

Denman Ross has written that the problem of art is to "aim at order and hope for beauty,"[15] but the achievement of order in an effective product of art is naïve, not rational. The criterion of both artist and observer is esthetically felt, but need not be intellectually known. As Leibnitz once remarked: "If I can recognize the thing, I have a clear or vivid knowledge of it, but this may be either confused (sensuous) or distinct (intellectual). It is confused if I cannot enumerate one by one the marks which suffice for distinguishing the thing from others. . . . Thus we see painters and other artists well enough aware what is right and what is faulty, but often unable to give any reason for their taste; if asked, they reply that the work they dislike lacks a *je ne sais quoi.*"[16]

Since the intellectually known is always superimposed upon that which first of all is esthetically felt, an experimental study of beauty should begin with something that is beautiful. The problem is then to discover the system of its harmony. Static symmetry is a systematic arrangement of recognizable units. It is, perhaps, the simplest order of spatial composition. But it is strictly limited to the combination and recombination of parts which readily disintegrate whenever the parts are too obvious, and which fail to impress us when they are merely the astract parts of a scale of measurement. Dynamic symmetry, in turn, is a systematic progression of the members of a whole. A pattern of this order, when suitably composed of recognizable

[15] *A Theory of Pure Design*, 1907, 5.
[16] Quoted from E. F. Carritt, *Philosophies of Beauty*, 1931, 57.

members, possesses the internal harmony of nature to which we are adapted because its system is our own.

What we have chosen to call naïve geometry is the immediate guidance of behavior by the system of space-relations in which we live. This system accounts for our first intuition of static or dynamic symmetry. To this system of naïvely apprehended summations and progressions we must turn for an explanation of those visible records of life which are called beautiful.

# THE BRAIN CONSIDERED AS AN ORGAN: NEURAL SYSTEMS AND CENTRAL LEVELS OF ORGANIZATION

By JAMES W. PAPEZ, Cornell University

The physical environment impinges upon the organism producing therein certain activities which lead to organismal performance and product, much of which is of a motor, visceral and psychological nature. This triple entente between environmental stimuli, internal organismal activities, and functional products may be represented by three sectors of a circle, the essential one of which is provided by the organism. This paper deals with the general plan of structural and functional organization of the nervous system which brings the organism into relation to the physical environment, on the one hand, and which, on the other hand, makes organismal performances and products possible, thus bringing the organism back into a retroactive relation with the environment.

Although neural systems both afferent and efferent are well known, the general nature and interrelations of neural levels are not clearly defined.

The term 'level' was introduced by Hughlings Jackson to imply stages of complexity of the sensori-motor functions of the central nervous system.[1] The concept of neural levels is frequently used to indicate successive steps in the organization of the nervous system or of its functional processes. In a measure, this concept has replaced the older notion of numerous centers dealing with specific functions. The uses of the term 'level' and their implications vary greatly. The meaning of 'reflex' level and 'cortical' level is generally understood, but the intermediate levels are not well defined and there is no clear recognition of their interrelations. The purpose of this paper is to indicate a number of the natural levels common to both afferent and efferent pathways within the central nervous system, and to point out that the cerebral cortex, which stands at the head of these levels,

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[1] Hughlings Jackson. On the relations of different divisions of the nervous system to one another and to parts of the body, *Brit. Med. J.*, 1, 1898, 65; H. Head, Studies in neurology, 1 and 2, (Collected works, 1920, 1-862, see Physiological Levels in index); R. S. Creed, The physiological integration of sensory processes within the gray matter of the nervous system: A critical review, *Brain*, 54, 1931, 29-53.

possesses also a recurrent relation to them which provides for an extensive recircuiting of central activities.

## Neural Systems

The classical dichotomy of the nervous system into afferent and efferent zones has been useful to the morphologists. It emphasizes the fact that afferent systems arise and enter into correlations in the dorsal half of the neural tube, while the efferent systems and the motor nerves upon which they converge are in general developed within the ventral half of the neural tube. On account of the interrelations which exist between the afferent and efferent zones at various levels, the dichotomy of these zones has a limited application. Numerous efferent functions have their inception in the afferent zones. The cerebellum, *e.g.* which is developed in the afferent zone, deals chiefly with efferent functions. These interrelations between the afferent and efferent zones will become clear as the various levels are discussed.

*The afferent systems.* The receptive functions of the organism are arranged into a number of afferent systems developed from the dorsal zone. Each system is equipped with special kinds of receptor organs attuned to respond to various kinds of environmental stimuli such as contact, temperature, damaged tissue, pressure, static displacements, sound waves, light, and stimuli of chemical nature. By the peripheral nerves each receptive field is converged into a complex conduction-system whose peculiar pattern provides the organism with specific sensibilities related to specific stimuli. On the way from the receptor organs to the higher levels of the brain each of the afferent systems makes a variety of appropriate connections with the various levels of the nervous system; with the motor nerves, with the cerebellum, the tectum opticum, the thalamus, the hypothalamus, the corpus striatum, and the cerebral cortex. Although the ultimate destination of each specific excitation is the cerebral cortex, subcortical connections provide each afferent system with a number of 'reaction' levels which may determine its central distribution, its blends and its attributes, and also the predispositions, tendencies and attitudes of response which may ensue.

Such afferent processes may be thought of as complicated neural replicae of the environmental stimuli which are acting on the end organs. They are by their very nature energy patterns within the nervous system which ascend to the level of the cerebral cortex. On account of its uniform structure and great expanse, the cortex more than any other level provides for a common meeting place for all of the afferent conduction systems. The cortex, however, is not a simple receptacle. In addition to being a receptive level it also emits fibers which reach down into the subcortical levels and afferent systems, without, however, involving the receptorial sources. This peculiar recircuiting, proceeding from the cortex to the lower levels and back again to the cortex, adds a recurrent, internalized process, reproduced along the lines of the primary afferent and efferent conduction patterns.

In general, eight afferent or externalizing systems can be identified: (1) the proprioceptive system; (2) the vestibular system; (3) the cutaneous system ex-

clusive of the face; (4) the trigeminal system from the face; (5) the acoustic system; (6) the ocular system; (7) the visceral afferent system; and (8) the olfactory system.

*The efferent systems.* The motor and visceral activities of the organism are carried out through the ventral half of the nervous system within which two efferent systems are constructed. One regulates the skeletal muscular system with which the organism confronts its external environment. The other deals with the internal systems of organs by means of which the organism regulates its internal environment. Corresponding to these internalizing systems there are the two major divisions of the body, the muscular apparatus and the autonomic apparatus, the latter including the visceral organs, the vascular system, and the glands. In such peripheral structures as the muscles, which are discrete motor units, there must be a coördination of activity which will lead to an integrated performance under a variety of conditions affecting the organism. This is equally applicable to the performance of the internal organs. Conduction within the motor (Fig. 2) and autonomic systems is, in general, deployed along reciprocal lines.

The effector side of the nervous system may, therefore, be separated into two general systems: (1) the motor system, which includes the motor nuclei of the spinal cord and brain stem, the corpus striatum or synergic center, the tegmental region, the cerebellum or energizing center, and certain cortical areas; and (2) the autonomic system, which includes the sympathetic system, the parasympathetic system, and the hypothalamus.

These two systems, the motor and the autonomic, are, however, not separated from the afferent systems. As a matter of fact, they have their inceptions in the various levels of the afferent zone. As a result of this interrelation between the afferent and efferent zones, there are formed a number of natural levels which in a large measure are common to both.

## Levels of Neural Organization

Each sensory system has its origin in more or less specialized or isolated receptors or sensory endings. Within the spinal cord and brain, each of the eight afferent systems makes seven typical connections with seven highly specialized regions or levels. These levels may be designated as: *(1)* reflex, *(2)* striatal-pallidal, *(3)* cerebellar, *(4)* hypothalamic, *(5)* tectal, *(6)* thalamic, *(7)* cortical. Each of these levels presents a distinctive organization of its own. At each level there is a different arrangement of neurons. They may be spread out in layers as in the cerebellum, tectum and cerebral cortex, or stretched into columns as in the spinal cord, or they may be massed into nuclei or cell groups as in the thalamus with specialized intrinsic and extrinsic connections. Associated with these architectural features are peculiarities of neuron form and cytology, recognized as cell types. On account of such structural conditions, each one of these regions may be regarded as representing a qualitatively different neural activity or as a region of special internal organismal response.

Each level is not, however, strictly limited to the organization of afferent impulses. As a matter of fact, it makes direct or indirect connections with some level of the motor or autonomic systems. Thus there exists at each level an afferent-efferent relation which is responsible for certain motor or internal predispositions. Such structures as the corpus striatum, the cerebellum and the motor nerves deal predominantly with motor functions. The hypothalamus, vagal structures, etc., deal predominantly with autonomic functions. In other regions, *e.g.* the spinal cord, tectum, thalamus, and cerebral cortex, the motor functions have their inception in structures which are predominantly on the afferent side. On account of this relation which each level of the central nervous system bears to the motor and autonomic apparatuses, the seven regions may be spoken of as neuromyonic levels or sensori-motor sectors. Their hybrid afferent-efferent nature can be appreciated by comparing Figs. 1 and 2.

It should be recognized, however, that the various afferent systems as they proceed from their receptors to these several levels are not conglomerate. They differ, one from the other, in certain important respects. To begin with, their receptors are different, responding in most instances to special forms of physical energy. Their central connections differ, being represented in many instances by specialized cell masses or nuclei differing in the number, size and cytological character of the cells and in the caliber, length and mode of termination of the nerve fiber. These differences culminate at the level of the thalamus where large nuclear masses are differentiated in special relation to each of the main afferent systems. Beyond the receptors and afferent nerves, separate nuclei and fiber tracts are thus formed which, taken in their totality, represent a specialized conduction system. A high degree of individuality exists in the central makeup of such externalizing systems as the proprioceptive, cutaneous, visceral, vestibulbar, acoustical, ocular and olfactory. This probably accounts for the high degree of local and qualitative specificities.

In spite of the great divergence of organization which exists in the different afferent systems, they all make connections in their own peculiar way with the seven neural levels. This means that there is an internal patterning of neural process common to all sensory avenues regardless of their receptorial source or individual architecture. This implies that, whatever the source or character of the stimulus or the structure of the afferent pathway, the internal organismal sequel assumes a concordant pattern. In this common process the activities of all the neuromyonic levels participate, each as a part-event, all being integrated into a unified or total reaction. The connections which exist between these different levels bind their activities into

an aggregate to which the architecture of each level contributes a neural process of its own peculiar quality.

As an example of the afferent connections with the seven neuromyonic levels, we will take the proprioceptive system. It should be understood, however, that the other seven afferent systems connect with these same seven neuromyonic levels each in its own way.

*(1) The reflex level.* Direct afferent-efferent connections are established in the spinal cord and brain stem which represent the reflex levels of all afferent systems. In this case the proprioceptive receptors located in the muscles, tendons and ligaments, are connected with the cord by their afferent nerves. Within the cord they enter into connections with certain motor and intermediate neurons, through the medium of which an appropriate reflex movement may be evoked by the proper stimulation of the receptor organs.[2]

The reflex is, of course, only a part-event in the more generalized radiation of the excitation process which spreads upward to the other levels of the nervous system. As a result of the same adequate peripheral excitation, a number of circuits at various levels of the nervous system are brought into operation, each of a special nature, depending on its own organization. Of this totality of central process the reflex is but a fractional increment, seldom, if ever, operating alone. Only under experimental conditions, such as severing of the cord, can its isolated operation be demonstrated. Such isolated activity, is, however, not native to the intact organism. Concurrent activities at higher levels represent the normal current of events.

Owing to the predominance of the reflexive component in some activities and also to the fact that they can be studied as isolated processes, it is often possible to define the 'reflex' connections as well as the level in which they occur. As a rule, the stimulus arises from a large area so that many receptors are involved. A large number of peripheral as well as central neurons are thus involved in a reflex reaction. The reaction is usually total for the limb or organ concerned; and the response is usually appropriate, *i.e.* it is said to be useful to the organism. This reflex response depends upon an inherent organization within the cord. The notion of a single receptor and point for point reaction is a physiological abstraction, the aim of which is to schematize the concept of the reflex arc and reduce it to simple terms. Any reflex is, as a matter of fact, a complex and somewhat labile neural process depending upon a central structural organization inherent in the spinal cord or some other part of the nervous system. Upon this labile organization its variable physiological response depends. In some instances, as in the muscular system, the reaction may be quite variable. In other cases, as in the optical system, the response may be the same under varying conditions of stimulation. In still other cases, as in the emptying of the bladder, both fields, *i.e.* of stimulus and of response, are reduced to narrow limits. We cannot restrict the reflex concept to arcs of two single neurons; for the

[2] C. S. Sherrington, *Integrative Action of the Nervous System,* 1906, 1-411; R. Magnus, Physiology of posture, *Lancet,* 211, 1926, 531 and 585; and *Körperstellung,* 1924, 1-740; C. L. Evans, The mechanism of postural reflexes and the functions of the labyrinth, *Recent Advances in Physiology,* 1926, Chap. 13, 305-333; J. F. Fulton, *Muscular Contraction and the Reflex Control of Movement,* 1926, 1-644; and F. Fearing, *Reflex Action,* 1930. 1-350.

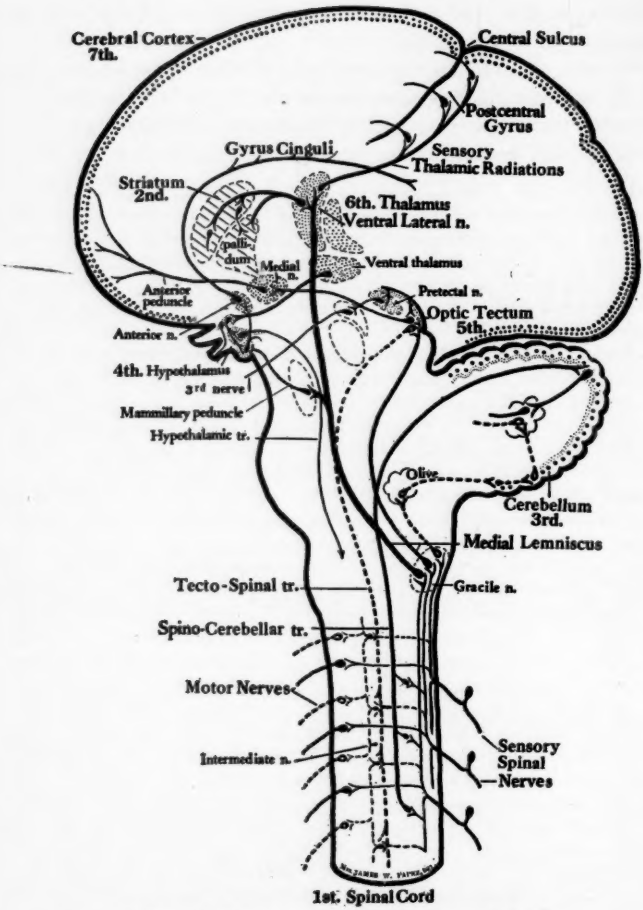


FIG. 1. DIAGRAM OF THE PROPRIOCEPTIVE SYSTEM
This diagram shows the connections with seven levels typical of the nervous system: (1) spinal cord or reflex level; (2) corpus striatum or synergizing level; (3) cerebellum or energizing level; (4) hypothalamus or autonomic level; (5) tectum or primary optic level; (6) dorsal thalamus or amplifying and distributing level; and (7) cerebral cortex or patterning, aggregating and recircuiting level.

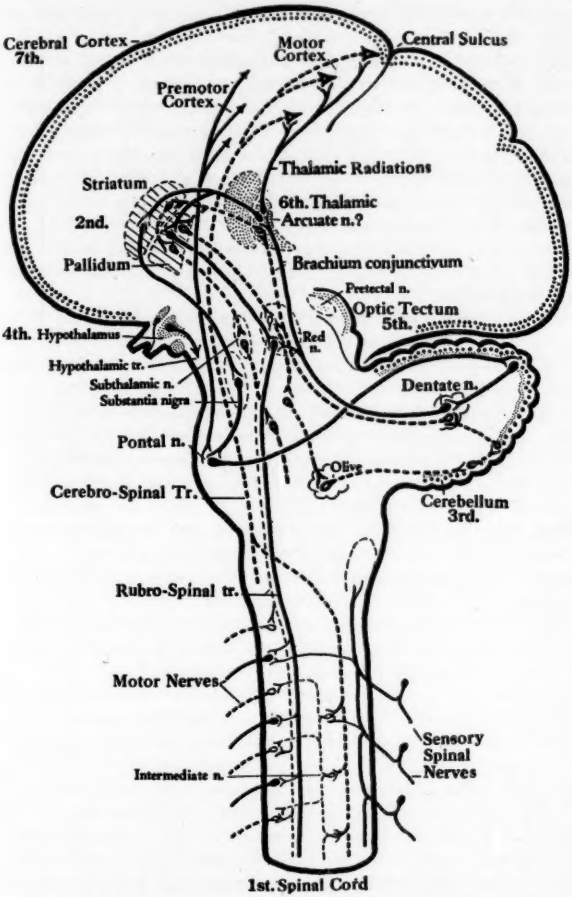


FIG. 2. DIAGRAM OF THE EFFERENT SYSTEM
This diagram shows the interconnections of the various levels of the efferent system which are the same as in Fig. 1. Broken lines indicate reciprocating lines of conduction. n = nucleus, tr = tract. Of the projection tracts from the cortex only the cerebro-spinal and cerebro-pontal tracts are shown.

labile reflexes a multiplicity of neurons as well as a central organization are essential.

*(2) The striatal level.* Long distance connections to the corpus striatum are established by all of the afferent systems through the medium of the dorsal thalamus. These connections may be illustrated by the spinal proprioceptive nerves which terminate by long ascending fibers in the nucleus gracilis and nucleus cuneatus at the base of the medulla oblongata. From these nuclei arise the fibers of the medial lemniscus which ascend to end in the ventral lateral nucleus or pars externa of the ventral nucleus of the thalamus. From this nucleus fibers pass to the pallidum and striatum as the thalamo-striate radiations.

Here the general patterns for complex, coördinated movements are deployed into various fiber channels which descend through the tegmentum and ultimately reach the lower motor centers.[8]

For this reason the corpus striatum may be regarded not only as the head coordinator for general responses of the motor apparatus, but, in coöperation with the cerebellum, as the chief synergic center for the regulation of reciprocating muscular action, commonly known as automatisms. Through the ascending paths of the various afferent systems it receives informative impulses which release appropriate avenues of reaction. From the cerebellum it receives the energizing impulses necessary to augment the volume of nervous discharge required for the vigorous, fast or slow performance of muscular movement. The cerebellum provides the energizing factor. Through the medium of the cerebellum the striatum provides the synergizing factor, while the local reflex connections provide the final mechanisms which activate the musculature. Within and between these levels, there exist reciprocal pathways which enable the various opposing muscles to act as reciprocating groups. The Vogts have ably sponsored this view of central automatisms.

*(3) The cerebellar level.* The various afferent systems enter the cerebellum chiefly through the inferior cerebellar peduncle or restiform body, but those of cortical and striatal origin enter through the pontal nuclei, and the brachium pontis. In case of the proprioceptive system, the spinocerebellar tracts and the fibers from the lateral cuneate nucleus constitute the special afferent paths. Special branches (collaterals) from the lower spinal proprioceptive nerves end in the dorsal nucleus or column of Clarke. From the cells of this nucleus arise the fibers of the dorsal spinocerebellar tract which conducts the proprioceptive impulses from the lower limbs and most of the trunk upward to the cerebellar vermis. The proprioceptive fibers from the upper two thoracic nerves and from the cervical nerves, including the brachial plexus, terminate in part in the lateral cuneate nucleus. From the cells of this nucleus fibers pass into the restiform body and end in the cortex of the cerebellum. They conduct to the cerebellum proprioceptive impulses from the upper extremity, the neck, and the two upper thoracic segments.

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[8] C. Vogt and E. Vogt, Zur Kenntnis der pathologischen Veränderungen des Striatum und des Pallidum und zur Pathophysiologie der dabei auftretenden Krankheitserscheinungen, *Sitzber. Akad. Wiss., Heidelberg,* 1919, Abt. B, Abh. 14, 1-56; A. Jakob, *Die extrapyramidalen Erkrankungen,* 1923, 1-420; S. A. K. Wilson, Progressive lenticular degeneration, *Brain,* 34, 1912, 295-509; *ibid., Modern Problems in Neurology,* 1929, 1-364; J. R. Hunt, Progressive atrophy of the globus pallidus, *Brain,* 40, 1917, 58-148; P. Foix et J. Nicolesco, *Les noyaux gris et region mesencephalic sous-optique,* 1925, 1-582; and L. O. Morgan, The corpus striatum, *Arch. Neurol. & Psychiat.,* 18, 1927, 495-549.

The cerebellum may be regarded as the chief energizing center for the proprioceptive and other systems.[4] Energizing functions are inherent in all parts of the central nervous system wherever impulses deploy widely over numerous paths. This function of energizing is, however, elaborated in a remarkable way in the cerebellar cortex where great folded fields of Purkinje cells stand ready to discharge their energy at the demand of various systems. The volume of the impulses destined for various muscular groups is probably regulated by the size of the cerebellar lobules. This discharge is carried out through the basal cerebellar nuclei and their tracts, the fastigiobulbar tracts and the brachia conjunctiva. These tracts energize three important levels of the motor apparatus: The vestibular reflex centers, the corpora striata, and the motor cortex, all of which are active in regulating movements and the postural adjustments of the body and limbs. This energizing function of the cerebellum enables us to think of it as being the power house for the development and maintenance of nerve impulses along diverse, long and complicated lines of conduction in the central nervous system and in the peripheral nerves. This dynamic vigilance is not a function of local origin, for it is only invoked through the activity of an afferent system, corpus striatum, tectum or the cerebral cortex. Moreover, since muscular action is in almost all parts of the body of a reciprocating nature, it follows that the organization of the cerebellar connections with the corpus striatum and with the vestibular apparatus is in the nature of reciprocal pathways of conduction.

*(4) The hypothalamic level.* The connections of the various afferent systems to the hypothalamus have not as yet been fully recognized. They are effected through the various primitive nuclei in the subthalamus (ventral thalamus) and by means of the various supraoptic decussations which join these centers to the hypothalamus. The antiquity of these centers is well known to the comparative neurologist. Long ago Claude Bernard recognized the functional relationship of the hypothalamus to the internal milieu of the organism. The part it plays in the wisdom of the body is by no means a new or undefined concept. Through the pars ventralis of the lateral geniculate body the optic system is joined by means of Meynert's commissure with the hypothalamic nuclei. Through the pars ventralis of the medial geniculate body the auditory and vestibular systems are joined to the hypothalamus by means of the two ventral supraoptic decussations. For the olfactory system we have the medial forebrain bundle and the terminal stria ending in relation to the hypothalamic, preoptic, and supraoptic nuclei. For the proprioceptive system the case is not so clear. The mammillary peduncle comes from the region of certain tegmental centers related to the medial lemniscus[5] and may conduct proprioceptive impulses to the mammillary body. It is known that the proprioceptive functions have a marked influence on cenesthesia. This is defined as a 'general sense of bodily existence' ultimately derived in part, as we may suppose, from the functioning of certain internal organs.

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[4] L. Luciani, *Il cerveletto,* 1891, 1-320; and *Human Physiology,* (3d ed.), 3, 1915, 419-484; L. Bolk, *Das Cerebellum der Säugertiere,* 1906, 1-337; A. Thomas and A. Durupt, *Cerebellar Localization,* 1914, 1-194; F. Tilney and H. A. Riley, *Form and Function of the Central Nervous System,* 1921, 407-484; F. R. Miller, Physiology of the cerebellum, *Physiol. Rev.,* 6, 1926, 124-159; and G. Holmes, The symptoms of acute cerebellar injuries due to gunshot wounds, *Brain,* 40, 1917, 461-535.

[5] L. Edinger and A. Wallenberg, Untersuchungen über den Fornix und das corpus Mammillare, *Arch. Psychiat.,* 35, 1901, 1-21; J. W. Papez, The thalamic nuclei in the nine-banded armadillo, *J. Comp. Neurol.,* 56, 1932, 49-103 (esp. 98); and *ibid.,* The mammillary peduncle, Marchi method, *Anat. Rec.,* 25, 1923, 146.

The hypothalamus may be regarded as the highest center of correlation for the autonomic system under normal bodily conditions.[6] The pars optica through its connections with the hypophysis regulates water metabolism and other related functions. The tuber regulates the functions of the sympathetic system, some of which the neurologist likes to regard as 'emotional expression.' The pars mammillaris should be involved in the psychologist's emotive functions. The hypothalamus is the recipient of those widespread and ill-defined cenesthetic excitations which arise in association with all sorts of visceral, cerebral (*e.g.* hippocampal), and other somatic activities, activities representing, in their totality, afferent material used by the organism under common conditions, as well as in more unusual ways (*e.g.* in problematical, dangerous, or predicamentive situations). On account of its connections from the ventral thalamus (subthalamus) the hypothalamus may be regarded as a center of vigilance with respect to external stimulating agents, a center which also regulates accordingly the internal environment in unusual ways by producing significant bodily and cerebral changes in critical situations. It is not difficult to see that this region stands related on the psychological side to the emotive functions, which involve in a significant way also cortical areas.[7] The mammillary bodies form an important part of the hypothalamus. Each mammillary body is connected forward by a conspicuous bundle, the mammillo-thalamic tract or fasciculus of Vicq d'Azyr, to the anterior thalamic nuclei. These nuclei are in turn connected forward with the cortex of the gyrus cinguli, which forms a conspicuous feature on the medial surface of each cerebral hemisphere. It is for this reason that the cortical repercussions of the hypothalamus should be referred to the medial surface of the cerebral cortex.

*(5) The optic tectum.* This structure receives connections from every afferent system. In case of the proprioceptive system such a connection is made by fibers of the medial lemniscus which turn dorsally and end within the middle layer of the tectum. This region of the brain is the primary central station receiving retinal excitations from spatially patterned photic stimuli.

The optic tectum (superior colliculus or quadrigeminal body) is primarily an optic center in the dorsal side of the mid brain, in which the primitive portion of the optic nerve ends. Since these fibers are distributed over the tectum from front to back in a topographic way it follows that the retina has a topographic repre-

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[6] R. Greving, Die zentralen Anteile des vegativen Nervensystems, *Hdbh. d. Anat. d. Menschen,* Bd. 4, 1928, 917-1060; W. B. Cannon, *Bodily Changes in Pain, Hunger, Fear and Rage,* (2nd ed.), 1929, 1-404; P. Bard, Emotion: 1. The neuro-humoral basis of emotional reactions, *Handbook of General Experimental Psychology,* 1934, 264-311; J. Beattie, G. R. Brow, and C. N. H. Long, The hypothalamus and the sympathetic nervous system: The Vegetative Nervous System, *Proc. Assoc. for Research in Nerv. and Ment. Dis.,* 9, 1930, Chap. XI, 249-316; J. F. Fulton, New horizons in physiology and medicine, the hypothalamus and visceral mechanisms, *New England J. Med.,* 207, 1932, 60-68, 94-95; S. W. Ranson, The hypothalamus; its significance for visceral innervation and emotional expression, *Trans. Coll. Phys., Phila.,* 2, 1934, 222-242; and A. R. Vonderahe, The representation of visceral functions in the brain, *Ohio State Med. J.,* Feb. 1935, 1-6.

[7] L. Haskovec, Le psychisme sous-cortical, *Revue Neur.,* 32, 1925, 976-988; *ibid.,* Nouvelles contributions au psychisme sous-cortical, *L'Encephale,* 24, 1929, 846-855; M. Nathan, Le troisieme ventricle, constitue-t-il un cerveau affectif?, *La Presse Med.,* 39, 1931 (no. 46), 857-858; and J. W. Papez, A proposed mechanism of emotion, *Arch. Neurol. & Psychiat.,* 1937 (in press).

sentation in the optic tectum providing the tectum with an excitatory field which generally duplicates the order of the outspread retina. It seems reasonable to suppose that this arrangement provides the organism with the subcortical mechanism concerned in the visual apprehension of position and movement of objects.

Beneath the anterior part of the tectum there are a number of pretectal nuclei upon which collaterals of the optic nerve and fibers from the optic tract converge. These pretectal nuclei are in turn connected by fibers with the ciliary, pupillary and oculomotor nuclei of both sides; that is, by pretectal tegmental fibers to the nuclei of the same side and by the posterior commissural fibers to those of the other side. Through these sets of fibers the pretectal region is able to effect pupillary and ciliary changes within the eye as well as ocular movements by way of the extrinsic ocular muscles.

The functions of the pretectal centers which subserve the optic reflexes are accessory to those of the tectum. Their fiber connections preclude any point for point representation of these optic reflexes. The pretectal nuclei are diffusion centers which converge their fibers on the oculomotor, ciliary and pupillary nuclei in such a narrow way as to exclude spatial representation save through functional integration with the primary tectal and other regions.[8]

From each tectum there also arises a tectospinal tract, which passes down to the cervical and thoracic spinal cord. Appropriate movements of the head, neck and upper portion of the trunk in response to optic or other stimuli are here provided for. The optic righting reflexes are an example of this phase of tectal activity.

The posterior portion of the tectum also receives connections from the nucleus isthmi, a vestibular ('static sense') center, and from the ascending acoustic tracts. The interior of the tectum is connected forward by the internal medullary lamina and the lamellar nuclei with the medial nucleus of the thalamus. This in turn is linked forward to the corpus striatum and the frontal cortex. It seems quite possible that this system, with its cortical field in the frontal lobe and its radiation into the corpus striatum, is called into more elaborate function in the perception of bodily posture and orientation and with the continuity of neural events. A neural mechanism is thus constructed which, with its cortical representation, is most important in directing the movements of the organism and in maintaining a temporal continuity of neural events.

If the foregoing statements correctly predicate the uses of the optic tectum, it is clear to see why fibers subserving proprioceptive, cutaneous, acoustic, and equilibratory functions are so intimately connected with this optic level, upon which depend important orientational and temporal performances of the organism. These local tectal excitations are, however, only a part-event in a larger process of organization within any afferent system.

To avoid confusion, we must clearly separate the optic functions which depend on the tectum from the retinal functions which depend on the lateral geniculate body,

[8] S. Ramon y Cajal, *Histologie du systeme nerveux de l'homme et des vertebres*, 2, 1909, 174-226; C. Winkler, Le systeme nerveux optique, *Anatomie du systeme Nerveux, Opera Omnia*, 6, 1918, 37-121; J. W. Papez and G. L. Freeman, Superior colliculi and their fiber connections in the rat, *J. Comp Neurol.*, 51, 1930, 409-439; S. W. Ranson and H. W. Magoun, The central path of the pupillo-constrictor reflex in response to light, *Arch. Neurol. & Psychiat.*, 30, 1933, 1193-1202; and C. U. Ariëns Kappers, G. C. Huber, and E. C. Crosby, *The Comparative Anatomy of the Nervous System of Vertebrates, Including Man*, 1936, 1067-1100.

the occipital radiations and the occipital cortex. These latter structures more directly subserve the visual apprehension and character of objects. They represent a higher level of neural organization and process. Both parts of the ocular system are, however, excited by retinal stimuli and must be concurrent and concordant in any performance of the entire ocular apparatus. The occipital cortex projects fibers down to the optic tectum, especially to the pretectal nuclei.

*(6) The thalamic level.* The thalamus is represented by four parts, the dorsal thalamus, the ventral thalamus (subthalamus), the hypothalamus and the epithalamus. The epithalamus receives olfactory connections, while the hypothalamus constitutes a special level of the visceral system as already noted. We are here more specifically concerned with the dorsal and the ventral thalamus.

The dorsal thalamus is the chief internode between the brain stem and the cerebral hemispheres. All of the main afferent systems (excepting the olfactory) reach the corpus striatum and the cerebral cortex through the nuclear masses which form the dorsal thalamus. This internode serves as a medium through which proprioceptive, cutaneous, trigeminal, acoustical, ocular and visceral afferent excitations are grouped and relayed to the corpus striatum and to the various 'receptive' areas of the cerebral cortex. Some of the cell masses of the dorsal thalamus constitute the paleothalamus sending radiations mainly to the corpus striatum, others constitute the neothalamus sending radiations mainly to the cerebral cortex. The nature of any particular afferent system determines the proportionate volume of the radiations to these two levels.[9]

Three other purposes are served by the dorsal thalamus. (a) It provides for a general amplification in the number of fibers in each of the afferent systems, thereby increasing the complexity of the conduction pattern. In some cases, (b) it serves to separate more completely the afferent pathways, while, in other cases, (c) it seems to provide for intrathalamic blends. From the receptive areas of the cortex it receives the cortico-thalamic fibers which reach down into the thalamic nuclei and the receptorial systems and thus establish a recurrent (regurgitant) relation between the cortex and the lower afferent levels. The proprioceptive system reaches the ventral lateral nucleus of the thalamus which is connected upward with the corpus striatum and the postcentral gyrus of the cerebral cortex.

The ventral thalamus or subthalamus is an ancient region which receives the primitive connections from all of the afferent systems. It serves as an internode connecting these systems with the hypothalamus. Its structure and modes of connection probably determine in part the pattern of the more unusual and episodic hypothalamic reactions referred to above. The reticular nucleus is a large primitive subthalamic mass whose connections are widely dispersed to the cortex, a broadcast relation not shared by any other thalamic nucleus. In addition to these afferent connections the subthalamus contains certain motor stations and pathways related to the corpus striatum.

*(7) The cortical level.* The highest connections of all of the afferent systems are

[9] W. E. LeGros Clark, Structure and connections of the optic thalamus, *Brain*, 55, 1932, 406-470; J. W. Papez and L. R. Aronson, Thalamic nuclei of Pithecus rhesus, *Arch. Neurol. and Psychiat.*, 32, 1934, 1-44; G. C. Huber and E. C. Crosby, Somatic and visceral connections of the diencephalon, *Arch. Neurol. & Psychiat.*, 22, 1929, 187-229; and C. U. Ariëns Kappers, G. C. Huber, and E. C. Crosby, *loc. cit.*, 1100-1190.

made with individual receptive areas of the cerebral cortex, the most uniform and most extensive level of neural organization. Fiber connections with the cortex are not direct. The afferent paths reach the dorsal thalamus where a great increase in the number of neurons takes place and whence thalamo-cortical radiations conduct excitations from receptorial sources and from the lower levels to the cerebral cortex.[10] Proprioceptive excitations reach the cortex of the postcentral gyrus. The thalamic radiations determine the final disposition of any receptorial pattern which reaches the cortex.

The cerebral cortex has a unique structure whose peculiarities and relations to the lower levels are not shared by any other part of the brain. In structure it is a uniform blanket or mantle of nerve cell-bodies, with extensive interconnections.[11] It is divisible into two general strata; the supragranular cortex and the infragranular cortex. The afferent fibers of the diverse afferent systems are believed to terminate mainly in the outer stratum, the supragranular cortex.[12] Here irradiation and dispersion take place, which may play their part in distinguishing objects.

In contrast to the supragranular cortex which serves as a receptive layer for the afferent systems, for the several levels and for the cerebellum, the relations of the infragranular layer are reversed. Through the projection fibers which arise from the infragranular layer[13] the cortex possesses a unique recurrent relation to the other levels of the nervous system. These projection, or cortical efferent, fibers connect the cortex with the thalamus, hypothalamus, tectum, cerebellum, tegmentum and spinal cord. These cortical fibers reach down into the afferent systems as far as the thalamus and in some cases beyond. In this projection the receptorial structures and the distal parts of the afferent systems are not involved in any important way.

Of these projection fibers, the cortico-thalamic fibers, the cortico-hypothalamic fibers (fornix), the cortico-tectal fibers, the cerebro-spinal and the cerebro-pontal-cerebellar tracts are well-known bundles. The cortico-thalamic fibers reach down into all of the afferent systems. The fornix reaches down to the hypothalamus, and may thus be involved in emoving. The cortico-tectal fibers reach down into the optic tectum, coming into especial relation with the pretectal nuclei. The cerebro-spinal and cerebro-pontal tracts[14] reach down into the efferent systems. The cerebro-spinal and cerebro-bulbar tracts end on the intermediate cells (Schaltzellen) related to the

[10] S. Poliak, *The Main Afferent Fiber Systems of the Cerebral Cortex in Primates,* 1932, 1-370; J. Dejerine, *Anatomie des centres nerveau,* 2, 1901, 232-810; and J. G. Dusser de Barenne, Corticalization of function and functional localization in the cerebral cortex, *Arch. Neurol. & Psychiat.,* 30, 1933, 884-901.

[11] S. Ramon y Cajal, *Histologie du système nerveux,* 2, 1911, 519-890; A. W. Campbell, *Localization of Cortical Function,* 1905, 1-360; K. Brodmann, *Vergleichende Lokalisationslehre der Grosshirnrinde,* 1909, 1-324; C. von Monakow, *Die Lokalization im Grosshirn,* 1914, 1-905; G. E. Smith, A new topographical survey of the human cerebral cortex, *J. Anat. and Physiol.,* 41, 1907, 237-254; and C. von Economo and G. N. Koskinas, *Die Cytoarchitektonik der Hirnrinde des erwachsenen Menschen,* 1925, 1-258.

[12] J. S. Bolton, *Brain in Health and Disease,* 1915, 1-479; G. A. Watson, Histological basis of amentia and dementia, *(Mott's) Arch. Neurol.,* 2, 1903, 424-620.

[13] O. Foerster and W. Penfield, The structural basis of traumatic epilepsy and results of radical operation, *Brain,* 53, 99-119.

[14] J. F. Fulton, The interrelation of cerebrum and cerebellum in the regulation of somatic and autonomic functions, *Medicine,* 15, 1936, 247-306; Fred A. Mettler, Cerebral function and cortical localization, *J. of Gen. Psychol.,* 13, 1935, 367-401; and C. J. Herrick, *Neurological Foundations of Animal Behavior,* 1924, 231-309.

motor nuclei of cranial and spinal nerves. Those movements whose patterns are of cortical design are probably released by these projection systems. The cerebro-pontal tracts reach down to the cerebellum. Along this great pathway, impulses of cortical origin travel to the pons and to the cortex of the cerebellum. The dynamic processes released at the level of the cerebellar cortex are carried to the thalamus and the corpus striatum, where the general organization of the appropriate movement takes place.

It is highly probable that, by means of the various projection fibers, the infragranular cortex is able to initiate at various subcortical levels efferent or motor processes as well as recurrent incitations which are returned to the cortex by the afferent systems.

The psychological functions would seem to be largely sustained by the cerebral cortex, as the cortex is conditioned[15] and modified by the influx and outgo of excitations. On the side of influx stand all the externalizing or afferent systems which reach it from the dorsal thalamus and olfactory centers, all concurrent excitations which reach it from the reticular nucleus, all excitations which reach it from the hypothalamus, all the energizing impulses which reach it from the cerebellum and all the recurrent incitations, initiated by the projection systems at lower levels. In this way, primary excitations and recurrent incitations are projected on the cortex where they are brought together into large, intricate and shifting functional patterns. It is quite possible that these dynamics tend to build up certain chemical and morphological residues which continuously facilitate and modify subsequent experiences of receptorial and of central origin.[16]

It is clearly apparent that, although the cerebral cortex contributes functions of a special nature, it is in no way independent of the afferent systems, of the various subcortical levels, or of the motor and autonomic apparatuses. More than any other region of the brain, the cerebral cortex is the common meeting place of all neural processes. Its uniform structure argues strongly for global or totalizing functions of all processes, whatever their source.[17] This is true in spite of the fact that localization of primary receptive areas and the so-called 'motor' and 'premotor' areas has been established. This does not mean that the cerebral mantle is a homogeneous matrix. It is, after all, a tissue made up of many layers of cell-bodies with diverse and intricate fiber connections through and within which an indefinitely great number of diffusion patterns and totalizing processes may take place. It is generally true throughout the nervous system that the greater the local mass of nerve tissue involved, the greater are the fusing and totalizing functions. A functional continuity may thus be maintained. This applies in particular to the cerebral cortex, where numerous integrations may follow in the wake of afferent and central recurrent excitations.

It seems probable that the orderliness and the progression of the central processes which underlie perceiving, comprehending, remembering, imagining, emoving, thinking, and other psychological functions[18] depend in the main upon the way in

[15] I. Pavlov, *Conditioned Reflexes*, 1927, 1-430.

[16] L. F. Barker, The clinical diagnosis of internal diseases; nervous system, *Monog. Med.*, 4, 1916, 287-328 (esp. 301).

[17] K. S. Lashley, Mass action in cerebral function, *Science*, 73, 1931, 245-254; *ibid.*, Integrative actions of the cerebral cortex, *Physiol. Rev.*, 13, 1933, 1-42.

[18] M. Bentley, *The New Field of Psychology*, 1934, 3-31, 91-174, 319-351; *ibid.*, *The Problem of Mental Disorder*, 1934, 283-289.

which the afferent externalizing, and the recurrent internalizing, impulses enter and undergo mixtures and integrations at various levels.

## Summary

There is evidence from many sources, clinical, experimental, anatomical, physiological, and psychological, that the activity of every neural level is conditioned by a variety of factors at other levels, and that each of these related levels of organization has a distinctive contribution of its own to make to the larger total organization arising from the activities of all the levels concerned. Although a functional idiosyncrasy is assumed for each level, it would be difficult to believe that any one level could normally operate without the participation of all the other levels, which must be directly and simulataneously involved in concurrent neural processes. There are many indications of preliminary subcortical integrations, or the setting of nervous mechanisms, at the levels of the cord, corpus striatum, cerebellum, thalamus, hypothalamus, and tectum, which are the forerunners and determinants of the final act as regulated and determined in part by the cerebral cortex.

The question arises as to whether this interpretation is just another aspect of the 'reflex' theories so often invoked to explain the dynamic, totalizing functions of the nervous system. Emphatically no. In general, the reflex theories do not take into account any qualitative differences which exist at different levels as here described nor the recurrent relations between the cortex and the lower levels. Straight-line or reflex-arc conduction is the essence of the chain-reflex theories. In the various levels of neural activity described here conduction is not left out, nor are reflex organizations omitted or denied. What is added is a number of other levels of integration where, owing to the architecture and the mass-action of specially organized nerve tissue, a qualitatively different function is contributed to the total neural process. Each neuromyonic level may be regarded as a special organizer of one phase of the total process, contributing to all the others. If these several levels exist, it is evident that a final and common totalized process is the result, for the different levels are neither anatomically or physiologically separable, but must be regarded as interrelated regions.

On the receptorial side we have evidence that many structures—receptors, receptive nuclei, relay centers, dorsal thalamus, reticular nucleus and cortex—participate in the elaboration of specific materials for the apprehensive, executive, and other functions. And there seems to be little doubt that configurational patterns of all kinds emerge at the level of the cerebral cortex; and also that recurrent configurations of various kinds arise quite

unlike the afferent patterns. Yet the afferent and configurational patterns are linked to each other and to the motor patterns in a labile way so that prediction in terms of stimulus and of resulting organic activity, physiological and psychological, cannot be made with certainty. In the motor field we have evidence that many structures below the cortex participate in the activity without actually determining the specific pattern of the skilled movement. Thus the reflex levels, the thalamus, the corpus striatum, and the cerebellum contribute to the execution of a skilled movement whose details may be more broadly and precisely patterned in the infragranular cortex, especially in the motor and premotor areas.

In the foregoing account the proprioceptive system has been used as an example to illustrate the typical connections of an afferent system with seven neuromyonic levels. This same plan of levels can be anatomically demonstrated for any one of the eight afferent systems. This has already been stated for the acoustic and trigeminal systems.[19] Some exceptions, of course exist and many special arrangements are found; but, on the whole, the levels and connections described here are the prevailing ones for all afferent systems as well as for the recurrent cortical connections. It seems reasonable to infer that the afferent systems represent the essential mechanisms for externalizing activities, while the recurrent systems, with the motor and autonomic apparatuses, represent the essential mechanisms for motor, visceral and all other internalized activities.

[19] J. W. Papez, Evolution of the medial geniculate body, *J. Comp. Neurol.*, 64, 1936, 41-61; J. W. Papez and R. W. Rundles, The dorsal trigeminal tract and the centre median nucleus, *J. Nerv. & Ment. Dis.*, 85, 1937 (no. 5), (in press).

# THE PSYCHOLOGIST'S USES OF NEUROLOGY

By MADISON BENTLEY, Cornell University

In the preceding article my colleague Professor Papez has given an integral account of the brain in operation. Unlike most functional descriptions of this complicated organ, his description omits psychological terms and concepts. The author describes the bodily activities of a bodily system; that is to say, he writes in terms of neural physiology. Papez has tried to bring neurological order and sense into an exceedingly intricate physical system by assigning a specific functional task to each of a limited number of regions or 'levels.' "Each of these related levels of organization has," as he contends, "a distinctive contribution of its own to make to the larger total organization arising from the activities of all the levels concerned." Thus the keynote to cerebral activity is made the proportioned and coöperative functioning of a number of specific and qualitatively unlike part-systems.

This integrative view of the brain escapes, on the one side, the notion of a linear series of impulses from receptor to effector, a notion common to the theory of 'cerebral reflexes' and the cruder SR forms of behaviorism. On the other side, the view escapes all those current phrenologies which find an 'associative,' 'thinking,' 'reasoning,' or 'attending' organ in the frontal lobes, 'perceiving' organs in certain occipital, temporal, and other gyres, 'will' in a dominating place, 'emotion' in thalamus or hypothalamus, and so on. Most of these phrenological accounts arise from the infusion of obsolete or fantastic psychologies into the rigidities of neural anatomy.

The first of these crude neurologies is satisfactory only to those psychologists who are content to match 'stimulus' to 'response' in a causal way; the second only to those who continue to use the ancient faculties of perception, memory, thought, emotion, attention, and the like, which the complacent neurologist has taken over from Gall, Spurzheim, Broca, and their long line of successors in the hazy field of psychophysiology.

It stands to reason that the descriptive psychologist who knows his own facts should make good use of *a neutral neurology, i.e.* an account of neural structures and activities which has not sought the support of mental agencies, forces, and faculties, filling its own bare interstices with 'consciousness,' 'sensation,' 'thought,' and 'emotion'; should indeed make better use of such a neutral account of a bodily system, with no interlarding from

* Accepted for publication December 20, 1935. The reader will readily infer the heavy debt due from the author to Professor J. W. Papez and to his article above.

phrenology, than he could make either of a reflexology borrowed from Spencer, Bechterev, Watson, and Pavlov, or of a description of the central nervous system which is half physiology and half psychology.

Rare is the psychologist who takes no responsibility for the bodily facts of structure, origin, development, and operation. Baffled by the intricacies of bodily process and of the homeostatic devices for a balanced control of life and conduct, he may envy the Christian Scientist who blandly recites his dogmas about the unreality of matter or the psychoanalyst who forsakes corporeal actuality for romances about the unconscious and its mechanisms; but he can scarcely emulate these easy methods of ignoring the body. Unless he is prepared to stand for an independent mind and a libidinous and misbehaving soul, he can scarcely regard his proper facts as unrelated to an organized and integrated body.

Just what use a given psychologist will make of a neutral neurology depends upon training and interest. He may set his facts in parallel with brain-processes and so afflict himself with an eternal mind-body problem; he may limit himself to equating stimulus with response; he may seek a formula for relating physique to character, diathesis, or mental disease; he may be content to record bodily accomplishment in the maze or puzzle-box; he may compare the behaviors of phyla or genera; he may factor performance by statistical means; or he may frankly join the neurologist in brain-dissection, in the tracing of tracts, or in the amplifying and recording of nerve and muscle potentials and encephalic 'waves.'

In all these ways are the psychologists of today making reference to the body and to its operations. In all of them, as the present writer inclines to believe, may a neutral neurology—or, more generally, a larger neutral somatology—be made to serve those who call their profession 'psychology.' In the present context, however, is to be considered only one single psychological use of neurology for its own sake and written in its own proper terms. The psychology to be assumed in the following brief description is that organized in terms of (1) the psychological functions, of (2) the products of these functions, and of (3) functional initiation, control, and government.[1]

[1] This envisagement of psychological fact has been most fully set forth by the author in his *The New Field of Psychology*, 1934. Provisional and partial expositions will be found in *Psychologies of 1930*, 1930, 95-114; *The Problem of Mental Disorder*, 1934, 282-289; Mind, body, and soul in medical psychology, this JOURNAL, 45, 1933, 577-591, and *A History of Psychology in Autobiography*, 3, 1936, 53-67. The view should not be confused with others of a 'functional' nature and especially not with the 'biological functionalism' of Angell, Judd, Carr, and others, which looks upon mind as an adapting and adjusting agent and governor of the organism. The view here presented does without mind and without the biologist's

## The Psychological Functions

Those functions which fall to the psychologist for description have been found to be reducible to the following primary activities in the developed organism. First, the apprehending group (perceiving, remembering, and imagining), secondly, the executive group (acting and emoving), and beyond these inspecting, comprehending, goal-searching and elaborative thinking.[2] These are neither faculties, agents, nor hypothetical powers, but modes of organic performance, of the same general order as secreting, respiring, digesting, circulating, and other physiological operations. They run their course, issue in products, and are variously combined, integrated, foreshortened, disturbed, and disordered. They rest upon bodily structures and resources, just as the physiological functions do. They imply no supporting 'mind' and no conscious or unconscious mechanisms. The psychological and physiological functions of the organism do not differ by the presence or absence of 'mind' or 'consciousness.' They rather present such family resemblances of functional mode, of end-product or issue, and of combination, as that the physiologist's interest, terms, and training fit him for the description of the one sort of performance, the psychologist's interest, terms, and training for the other. Since the days of Johannes Müller, Carl Ludwig, Flourens, and Claude Bernard, physiologists have not advanced, by the use of their own concepts and methods, one step toward a descriptive account of perceiving, remembering, or comprehending, and, on the other hand, purely psychological methods have not, in the same time, noticeably advanced one step toward the physiological processes in muscle, brain, and nerve. The division of labor in functional studies is natural and fair, but great gains are to be made by wise cross-reference in both directions. The two sciences of life can never be wholly separated because one and the same bodily system is responsible alike for physiological and psychological modes of activity.

Now before we can intelligently ask the living body how it perceives objects, imagines fictions, comprehends speech, emoves in trying situations, and thinks out plans for future conduct, we must dwell briefly upon the

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teleology of adaptation. It looks upon the living body as operating in two general ways, a physiological and a psychological way; it omits all mental agencies and powers, and it does not, therefore, speak of an 'introspective' method. It will be impossible in this article fully to characterize the psychological functions. Detailed descriptions are to be found in about a dozen major studies (most of them doctoral dissertations) which have come from the author's laboratory. For a critical review of the neurologies of Lashley and Herrick, see T. A. Ryan, Neural resources of psychological performance, this JOURNAL, 46, 1936, 193-220.

[2] Cf. *The New Field*, etc., esp. Chap. i. The natural relations obtaining among the functions and the order of their first appearance in life are indicated in Fig. 1, below.

characteristics of these operations, study the manner in which they turn out products in the form of objects, fictions, language, emotive habits, beliefs, thought solutions, knowledge, laws, customs, and all the rest of man's world, which is, every bit of it, produced by these same operations, and

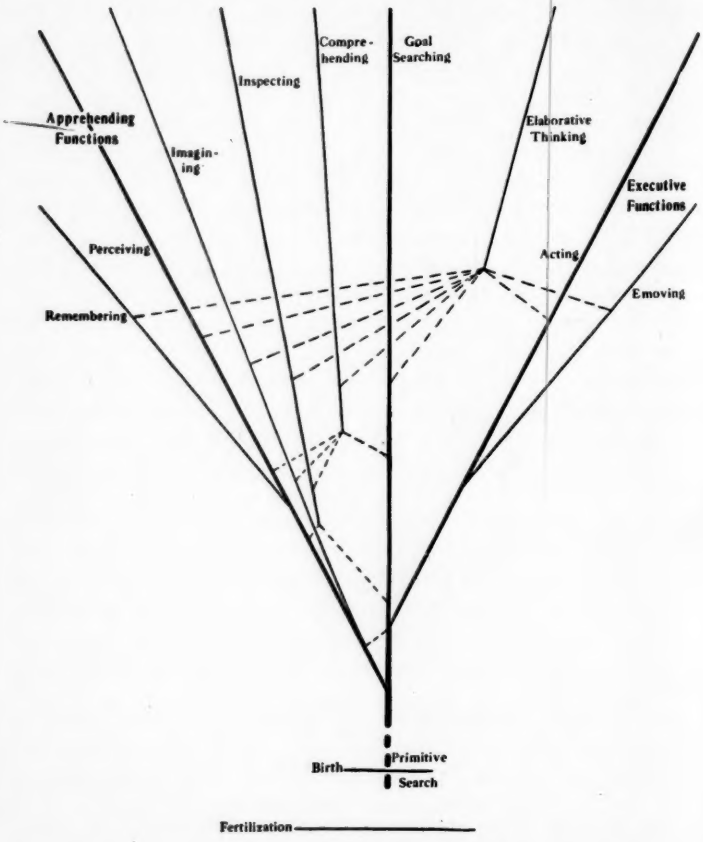



FIG. 1. THE PRIMARY PSYCHOLOGICAL FUNCTIONS

which stands to the psychologist's functions in just the same relation as gastric juice stands to the physiological processes of the stomach's wall, saliva to parotid activity, the egg to generative changes in the ovary, and bile to certain processes in liver cells.

We have no evidence of psychological functions occurring in man before or during birth. They appear in order after birth. The general order (though not the time-spacing) is indicated by the following figure.

The order is natural and fixed in the sense that one function depends upon the antecedent exercise of another. Thus the body must perceive before it can remember events as dated in the past or imagine a fictitious existence. The anticipative prophesy of action likewise implies both searching and perceiving, and emoving implies unimpeded action and a simple form of comprehending. The last type of function to come is elaborative thinking (laboring toward a thought-goal), for it rests upon the development and interconnection of all the others in the figure.

## A Neutral Neurology of the Brain

Now let us see how far a consistent neutral neurology, combined with all we know about the accessory bodily systems (muscles, glands, viscera, etc.), is able to take us toward an adequate understanding of the way in which the body thinks, inspects, understands, observes, remembers, and so on.

We take no account here of the high divergences among neurologists upon the range and limitations of function of the cerebellum, hypothalamus, prefrontal cortex, and other cerebral regions. For our present purposes, it is better to take one single descriptive account of the integrative functions of the brain and to make whatever use we can of it in psychology. Papez's fresh, factual, and authentic account is especially adapted to our ends. Moreover, it is not presumptuous to hope that a descriptive psychology which strives to be as independent of neurology *in the discovery and organization of its primary facts* as Papez has been independent of psychology may be more useful to the clinical and functional neurologist than his present appeal to psychological fragments; fragments taken out of traditional lore and out of the neurologist's uncritical and undisciplined use of such terms as 'emotion,' 'thought,' 'volition,' 'feeling,' 'attention,' 'personality,' and 'sensation.' This use bears no clear stamp of the proper or 'scientific' employment of that experimental method which the physiologist is himself careful to follow in discovering and testing his bodily facts.

It appears that the nebulous, confused, conflicting and contradictory descriptions of the mental states and powers for which this or that brain tract, nucleus, or organ is held responsible in neurological journals and in general works on psychiatry chiefly arise from an attempt to discover the cerebral seat of 'mental symptoms' and to explain the results of central lesion or surgical insult. There is no doubt that the diagnosing physician and the operating surgeon may sometimes be aided by reference to mental symptoms, no matter how crudely or inadequately these symptoms are described or named. Nevertheless, the relief of a symptom by a local excision should not be taken as proving that the excised bit is the 'seat' of a part or a characteristic of mind. As well might the Holland Tunnel be inferred to be the 'seat' of city shopping because a stoppage of travel there reduced the business of the midtown stores or spark-plugs regarded as the 'seat' of hill-climbing when an accumulation of carbon brought down the energy of the automobile to locomotion upon the flat plain.

Papez's primary point of emphasis is that the great afferent and efferent sides of the nervous system interlock in a fairly complicated way—not in a simple linear fashion—and are accessory to several nodal and zonal areas or 'levels,' each of which makes its own peculiar contribution to the functional integration of the whole central system of organs. These levels he names (1) reflex, (2) striatal, (3) cerebellar, (4) hypothalamic, (5) tectal, (6) thalamic, and (7) cortical. Each plays its own part, which accords with its cytological organization, its relations to the other zones, and, specifically, its particular afferent-efferent associations. (Cf. Papez, Figs. 1-2, pp. 222-223, above.) On the afferent side, the association is with certain receptors attuned to various forms of energy (light, sound, chemical changes, cutaneous pressures and temperatures, muscular contractions, visceral processes, glandular secretions, vestibular densities, and so on). Thus energies initiated outside and within the body attune these areas in many different ways and lead on locally to many patterns of neural activity. On the efferent side, as well, the association is specific. At the 'reflex' level, *e.g.* both in the cord and in the brain, certain conducting outlets lead immediately to selected gland- and muscle-groups, whereas the striatal level involves also cerebellar sources of energy and is itself a high coördinator. Other specificities in efferent function are similarly to be found at other levels.

Now since our psychological functions and products rest especially upon the integral and synthetic qualities of bodily operation, our interest will be centered in the *coöperative* possibilities of these levels or functional nodes. The possibilities suggested by Papez's plan are:

*(1) Reflex level.* In so far as the efferent discharge is directly to muscle, the psychological involvement is mediate, *i.e.* through the action of muscular contraction and proprioceptive excitation back to the central nervous system. These indirect effects may for the present be disregarded.

*(2) Striatal level.* Activated through the medium of the thalamus, the striatum is said to coördinate energies which derive ultimately from the receptors and so to pattern outgoing impulses as that the resulting movements are synergized in overt behavior. Since it adjusts movement mechanisms reciprocally to each other and arranges them in a temporally chained order, this level is obviously concerned in the executive functions, doubtless acting in a primary way for the smoother and more automatic actions, but probably depending upon the cortical (and perhaps thalamic) regions for aid in those prophetic actions where bodily movement and its immediate results in the outside world are anticipated by the organism.

*(3) Cerebellar level.* This area lies strategically for influencing the primary functional areas of the brain. On the afferent side, it is fed from proprioceptive impulses originating in muscle and joint and passing through the ascending spino-cerebellar and other tracts. It is aiso energized from the cerebral cortex, which seems to exercise

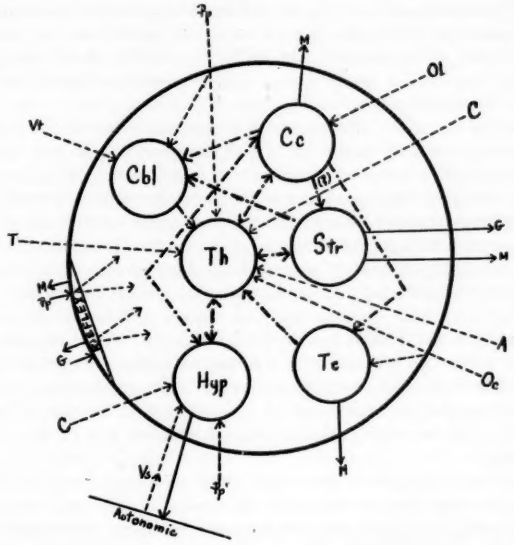
AFFERENT ASSOCIATES OF CNS

Pp = proprioceptive
Vt = vestibular
C = cutaneous
T = trigeminal
A = acoustic
Oc = ocular
Vs = visceral
Ol = olfactory

EFFERENT ASSOCIATES OF CNS

M = muscle effectors (striped and smooth)
G = gland effectors

End organ is reached by
(a) a motor or
(b) an autonomic pathway




←——— = Efferent polarity  - - - - - → = Afferent polarity  —·—·—·— = Interzonal relations (unipolar and bipolar)

ZONES OR 'LEVELS'

I. Refl = reflex
II. Str. = striatal
III. Cbl = cerebellar
IV. Hyp = hypothalamic
V. Tc = tectal
VI. Th = thalamic
VII. Cc = cortex, cerebral

PRIMARY PSYCHOLOGICAL FUNCTIONS

1. PS = primitive search
2. P = perceiving
3. M = remembering
4. Im = imagining
5. A = acting
6. E = emoving
7. In = inspecting
8. C = comprehending
9. ET = elaborative thinking
10. GS = goal searching

FIG. 2. A SCHEMATIC REPRESENTATION OF NEURAL ORGANIZATION

Main interzonal lines of central integration (Papez), with examples of afferent and efferent associates, and a list of the primary psychological functions.

a control over the motor functions of the cerebellum. Although the cerebellum probably does not directly discharge into motor tracts, it does exercise a profound influence by way of the red nucleus and the striate body. With the second (or striate) level, it is involved in the actional functions both in maintaining a base of 'tonic' contraction from which further contractions are initiated and in tempering the elements and phases of movement in pattern, strength, speed, and duration. So far as the cerebellum is dominated by the cerebrum, moreover, it doubtless plays its part in carrying out selected movements prophetically directed toward an action-goal. By some neurologists, a primary function of the cerebellum is held to be the maintenance and change of posture and of equilibrium. So far as this region is so employed, it will play a large part in the perception of bodily state, bodily movement, and bodily position. This employment would appear in rotation of the organism and in other disturbances of equilibrium.

*(4) Hypothalamic level.* Though mediately connected with various cerebro-spinal receptive systems, one main specific function of the hypothalamus seems to be related to the autonomic nervous system, for which it is said to be the highest center of correlation. As regards receptive systems, the neural connections, as well as clinical and experimental evidence, seem to indicate that many visceral and other bodily activities give rise, through the mediation of the hypothalamus, to what the physiologist has called 'cenesthesia' or 'general bodily feeling,' into which proprioceptive impulses (possibly conducted through the mammillary body, Papez) may also infuse a component representing muscular tone and muscular contraction. At any rate, this region seems to be involved in functions originating in visceral organ and skeletal musculature. It has also been urged of late that the region is functionally tempered by autonomic activities upon which it likewise exercises a regulative control. Whether and how this hypothalamic area serves in the psychological function of emoving, we shall have to consider when related regions of thalamus and cerebral cortex have been reviewed.

*(5) Tectal or quadrigeminal level.* First of all, this region provides a subcortical station for the optic tract, in which the outspread field of retinal receptors excited by light is reproduced. Here also originate efferent fibers controlling movements of the eyeball and contraction of ciliary and pupillary muscles. Fibers from the 8th nerve (both cochlear and vestibular fibers) indicate a participation of the tectal region in acoustic and equilibratory functions. Indeed, Papez's statement that the region is connected with *every afferent system* suggests its strategic position with regard to all excitations initiated by outside energies and by receptorial devices within the body, including muscles and semi-circular endings.

On the side of other central stations, fibers run from the tectum to the thalamus and thence to the striatum and the (frontal) cortex. On the efferent or motor side, tecto-spinal fibers from this region pass to the upper (cervical and thoracic) portions of the cord to effect movements of the head, neck, and upper trunk, movements which stand especially related to optical, acoustical, and vestibular matters. On the optical segment, descending fibers from the occipital ('visual') cortex to the tectum doubtless play an important part in moving and posturing the head and the upper part of the body in accord with impulses from eye, ear, and semi-circular canals, which are integrated in the tectal region. 'An important part' because simultaneous appeals from these sources, which have to do with bodily position and change and

with active objects outside, call for an integral and accordant response of the organism.

Although the optical relations of the tectal level are outstanding, the specific genius of this central area may rather pertain to its orienting facilities, in which, of course, the visual component plays an extremely large part; although the acoustical, vestibular, and proprioceptive connections are also important, on the one hand, and the tectal intimacies with cortex, thalamus, and striatum, on the other.

Such a neural nexus as the tectum is, in operations which have a highly organized and functional course (adjusting to the moving body and to moving objects seen and heard), may well be of primary use, as Papez has suggested, in the organization of temporal sequences in behavior. This possibility is to be kept in view in several of our psychological functions.

*(6) Thalamic level (dorsal and ventral).* All afferent systems, save the olfactory, pass through the dorsal thalamus on their way to cortex and striatum. The thalamus is the great internode and relay on the afferent side of the central nervous system. Its interposition provides an increase in number of fibers; it helps to separate and distinguish as well as to interfuse and blend the afferent pathways, and it establishes new (recurrent) relations between cortex and afferent structures by receiving from the cortex the cortico-thalamic system of descending fibers. The ventral thalamus is the part which serves as an internode between the afferent systems and the hypothalamus. It may help to organize, or at least to fuse, incoming impulses from visceral excitations.

*(7) Cortical level.* From the thalamic internode radiate to the various parts of the cerebral cortex the afferent fibers of the various receptorial systems; *i.e.* proprioceptive fibers to the postcentral gyrus, optical radiations to certain occipital regions, acoustical to a temporal area, and so on. Afferent excitations reaching any of these cortical stations may (1) suffer local irradiation and dispersion, (2) suffer transfer to other cortical areas (through association fibers), and (3) excite the cortex of the opposite hemisphere (through the commissures). Excitation from the outer (supragranular) cortex also (4) passes to the (infragranular) cortex and so (through projection fibers) (5) to pathways leading to thalamus, tectum, cerebellum, and spinal cord. Those fibers which proceed to the thalamus, hypothalamus, and other brain levels provide—in some instances—recurrent, return pathways which complete a double interacting system of excitations between the several lower levels and the cortex. Thus are functionally connected, in a great two-directional system, the various brain levels (probably omitting a cortico-striatal connection) which we have been considering. Other projection fibers reach down into the efferent systems, ultimately affecting muscle and gland.

This cortical field we can no longer regard as a final station or 'dead-end,' delivering 'messages' from the outside to a central mental agency which is capable of transforming such messages into resident thoughts, memories, emotions, and the like; nor as a great sluiceway opening into a precentral 'motor' funnel and thus making muscular and glandular responses to a 'stimulating' world which knocks at the gateways of 'sense.' It is rather another great nodal region designed, as Papez has put it, "to pattern, aggregate and recircuit," in a more commanding way than any of the other 'levels' and also with far greater lability and variety than these.

The degree of structural uniformity in the cortex has suggested "global and

totalizing functions" and the vast amount of interconnection within it and between it and all of the other levels speaks rather for integration and high participation than for local centers subserving this or that psychological function. Primary access from optical, acoustic, dermal, and other receptor origins, should not lead us to accord separate perceiving faculties to a visual, an auditory, a cutaneous, or a kinesthetic region of the cortex. At most these 'primary stations' can be responsible only for qualitative details in a total pattern which mediates, through the activity of the whole neuro-muscular system and accessory systems in the body, the apprehension of objects, scenes, changes, and occurrences.[3]

## Psychological Functioning and the Brain

When we ask the body quite concretely how it contrives to perceive, remember, emove, think, and the rest, we are confronted by a sobering and discouraging fact. Our present physiological knowledge of bodily systems, and notably of the nervous system, is quite inadequate to the derivation of satisfactory answers. Except for those who, impressed by a new method or a new theory, grow sanguine and confident, physiologists and psychologists alike stand ready to confess the inadequacy of our bodily knowledge. A fair example of this readiness is Professor Adrian's recent declaration that "we know far more about our thoughts and actions than we do about the mechanisms in our brain," (and again) . . . "in fact we know almost nothing . . . of the connection between mental and neural events."[4] It is instructive to reflect that the context of this confession of physiological ignorance was the issue of those recent studies on electrical potential in active neuron and muscle fiber, to which Adrian had himself made outstanding contributions. If the only moral to be derived from this limitation of the neurologist is to 'breed bigger (if not better) brains,' the psychologist must turn elsewhere for help upon the problem of how the body does what it is observed to do.

If we look to history for significant aid, we shall find that all the simpler answers to our enquiry have failed.

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[3] The writer has not ventured to go behind the preceding exposition of Professor Papez, which is well documented. Upon many of the details of Papez's integrational plan neurologists still differ widely. Into these points at issue we do not go: that is not the province of the psychologist. Furthermore, it is to be remembered that no topographic plan is here proposed or assumed. Fig. 2 is entirely schematic. Those nodal points to be abstracted from the whole brain have been set in the spatial order which is most convenient for indicating their functional interrelations.

[4] *Science*, 84, 1936, 275-278. The corollary to the admission of present ignorance is astonishing. Adrian faces complacently a possible *ignorabimus:* "Need we care greatly whether the neurologist can produce a scheme of nervous mechanisms which will account" for human behavior? He doubts whether "it would make us more effective units of society," and his only suggestion toward this great end is "to breed men with larger brains." "If our brains were but twice their present size, our behavior would then be superhuman." Do ancient vagaries *never* really die!

These include the neurological essays of the 18th century; the brains assumed by German rationalism, French sensationalism, the disciples of *Erfahrungsseelenlehre,* the physiological psychologists, and other simple proposals. Always was the neurological knowledge utterly inadequate. The phrenology established by F. J. Gall was little better, but it did set men diligently at work upon the brain to discover bit by bit and area by area how the thing served man. Its failure is measured by the agnostic declaration of Flourens that the brain thought, willed, remembered, and so on, as a whole and with few specific and local functions. A similar conclusion, chiefly based upon the rat, was recently reached (and partially abandoned) by K. S. Lashley. Flechsig translated English and German associationism into brain tissue and returned a crude answer to our question in terms of neurons and neuronal tracts, an answer which has probably retarded central physiology as much as any of the other, earlier theories. On one count, it led naturally and inevitably into the SR type of behaviorism, which gave small concern to the brain because it passed as rapidly as possible from the stimulated receptor to the responding muscle. The brain was thus reduced to a simple line-drawing which represented the stimulus coming in, making straight for the brain, whence it hurried out, zigzagging down the spinal cord and into appropriate muscles. This caricature also entertained the sensationalist who saw his sensations careening through similar pathways and taking their several posts in the 'sensory centers' of the cortex.

Clinician and pathologist have always supplied the counterblast to these easy and simple topographies of the brain; for their diligence in diagnosis has always been rewarded by 'mental symptoms' too complicated for words. The number of things which can go askew in mind when a bullet scratches the brain or a small tumor grows there is incredible. Thus have grown up in pathological neurology and neural surgery sundry doctrines of central seats of everything from a sensation to consciousness-at-large and personality. From the tangle of doctrines and speculations thus engendered, no one—so far as I know—has been able to separate out anything like a coherent and plausible account of the brain's functions. Of late, hope for this desperate state of things neurological has been sustained by new methods and new discoveries concerning humoral excitants, hormonal influences, and electrical potentials. So beautiful have been the new methods and so fascinating the results that we have for the moment tended to lose sight of the primary quest after the bodily means and agents which support those phenomena which men call—with countless shades of meaning—psychological.

It is not with an abundant courage or hope, therefore, that we approach our problem. I trust that we may agree, however, that the first necessity is a clear statement of, and a general agreement upon, the psychological facts for which we hold the body responsible. Let us repeat that, for our present purposes, we are to consider these facts as the psychological functions, their products, and their government. We shall see, then, how far

we can go at present in our search for bodily initiation and bodily support.

The whole task is plainly too heavy for the present undertaking. The limits of our search will not carry us much beyond a few samples of the functions, and samples of product and of governing agency.

## The Bodily Support of the Psychological Functions

First we must ask 'What are the fundamental characteristics of the psychological functions?' and 'What kind of bodily support do these characteristics suggest?'

The underlying characteristics are *determination* and *pattern*. A determined function implies a temporal course which is directed toward a natural end-term. Thus searching for a lost article, thinking through a problem in geometry, and straining to bring the skiff to shore in the tempest are obviously determined and directive operations. The other functions arranged in a natural order in Fig. 1 less obviously but just as truly are of the determined kind. Patterning is readily seen in perceiving a picture, in a rhythmical form, or a musical melody. Every element in the whole is set into its place: all are patterned together or in a fixed temporal order. Only so are the products of perceiving, remembering, and imagining created as the appropriate function is carried forward. We may, then, ask more specifically how the body is able to set up and to carry through determined and patterned operations.

A clear and significant example of the determined and directed course in bodily performance is the digestive cycle from pre-feeding and food-intake to elimination and cellular metabolism in the young infant. This example is especially appropriate for our purpose, moreover, because it seems best designed to prepare the body for, and to introduce, those operations which we call psychological.

Phases of this digestive sequence and sequel occur, to be sure, during fetal life, but it appears that the earliest of the psychological functions await the organic changes which come, after birth, with the establishment of post-natal circulation, respiration, food-intake, and the accompanying exercise of the general musculature. During the first days of the neonate, all of these bodily operations are progressively integrated into a unitary series of events. The bodily state before feeding runs into feeding (intake and ingestion), feeding into swallowing, swallowing into gastric processes, these into the multiple events of intestinal digestion and metabolism, these again into circulatory transport and tissue-change, as well as into the eliminatory processes which end the natural cycle. Into all stages of this cycle are beaten muscular tensions which emerge into coördinated and in-

coördinate movements of trunk, head, lips, hands, arms, and legs. Glandular changes and products are likewise integrated into all phases of the digestive event. In this way and with surprising speed is set up a ligated set of functional performances of the body, more and more closely bound into one event, and more and more 'pointing' throughout its course to the inevitable terminus. 'Pointing' here is first to be taken in a purely physiological sense. Each phase, so to say, advertises following phases and it is itself modified both by the earlier and the later phases, which together with it make up the unified physiological course of a complicated occurrence. The tough fiber of this integration is only realized when accident cuts across the course (*e.g.* premature withdrawal of the nipple or obstruction to swallowing), violently disrupting it and throwing the entire organism out of commission.

Now within this inclusive operation, which involves activities of all the great bodily systems, we find the prerequisite of our first psychological characteristic; namely, *determination*. If we consider the fixed and orderly phases of it and its directive aspect, we may look upon this basal organic operation as a kind of searching-out, but quite without prophesy of the end and quite without anything like 'knowledge' of the course. The term 'primitive searching' for this operation suggests that it lies on the borderline between the domain of the physiologist and that of the psychologist. It is not, of course, to be confused with wish, desire, drive, or libido. The first two of these call for an end of wish or of desire, the third is a modern variant of the concept of 'instinct' or else is an undetermined kind of causal energy, and the fourth is a speculative concept taken from a doctrinal setting which is remote from the natural sciences. 'Search' here implies no more than that the organism is going on in a fixed course (with many minor variations) and toward an inevitable termination and that, by virtue of its organs and tissues, its hormones, secretions, and other resources, the organism is in command of this single and integrated operation.

On the side of pattern, the parts and details are supplied first by all those elements in pre-feeding, feeding, metabolic change, etc., which make the functional phases specific. These elements may be likened to the several notes which make the melody when they are set in a certain order and given their respective temporal coefficients. The second and more 'picturesque' aspect of pattern is furnished by the neural receptors. These organs are incited to specific activity by specific forms of energy which act both at the boundaries of the body (in eye, ear, skin, etc.) and within its interior (end-organs in viscera, vestibule, and proprioceptive arrangements in the muscles).

Given these fundamental characteristics, let us consider some of the primary operations and see how far we can go in depicting the bodily organs and agencies which make the operations possible.

*Perceiving.* After primitive searching, which calls generally and widely upon the body, including neuro-muscular mechanisms but not necessarily cortical areas, we advance to apprehending, to the apprehending of objects, events, changes, and total occurrences, which are laid hold of, indeed created by, the living organism. Without doubt the form of apprehending which comes first in the individual life of the infant—as well as in the phyletic series of animal forms—is the apprehending of objects, and so on, as present and as going on now. That form we call perceptive apprehension. Let us take two relatively simple cases. I glance from the window and perceive a large building across the way. Again, I close my eyes and perceive a large bus rumbling below me on the street. How are these things done? There are two common, but wholly inadequate, answers. The first is that 'the mind' perceives building and bus: the second is that the eye sees and the ear hears. The one answer invokes magical agencies and powers as much as the other. The one generally satisfies the unreflective man and the philosopher; the other (provided we add optic tract and areas of cortex) the neurologist and the physiologist. Neither satisfies the psychologist, who studies and describes the perceiving event and then goes to the body without preconception and seeks the actual underlying conditions.

First observe that building and bus *are product, not condition,* of perceiving, not condition in the sense of something which affects and modifies the organism. Light rays in a certain pattern of energy and wave-length are disposed over the retina and so incite to activity a receptor apparatus: sound energies in simultaneous and successive arrangements of wave-length and amplitude incite receptive structures within the cochlea of the ear. Many bodily events follow. How do they contrive a perceiving?[5]

Here the neurologist seems to have been led astray by formulating his view of perceiving (he usually says 'perception' or 'sensation') from the camera-like eye. The hollow eye—he is wont to say—takes the picture and the impressible retina and cortex receive it and hold it. However physiological the neurologist may be in his bodily descriptions, he immediately becomes 'substantist' or 'structuralist' when he approaches the mental or the 'psychical.' Sensations and perceptions accordingly *are. 'To be'* they only require focusing camera and sensitive plate. Once this false solution is made in the convenient terms of vision, it is easily carried over to the other

[5] Cf. Bentley, *New Field,* 16-23, 91-95, 362-364; *The Problem of Mental Disorder,* 1934, 282-289; this JOURNAL, 45, 1933, 586-589.

sense departments. At the bottom of this naïve view of perceiving implicitly lies—of course—a magical 'mind-which-perceives.'[6] When we deny to the neurologist the borrowed conveniences of a magical mind, his snap-shooting eye and occipital cortex are seen to be another provision just as magical. Since there is no internal 'mental picture' and no corresponding static and frozen 'physiological state,' the riddle of bodily support for perceiving must evidently be solved—at least in part—in terms of an active, functional performance by the body which somehow represents the specific energies delivered to the receptor by the object-to-be-perceived.

Here we appeal to the principle of *mimetic duplication* or *functional mimesis*. Energies which originate in, or are reflected from, the object-to-be-perceived must somehow be duplicated within the body; not duplicated in the form of a 'mental image' within the brain but in terms of bodily function. Something goes on in the body which repeats those energy-patterns characteristic of the object-to-be-perceived; *e.g.* the outspread and contour of light energies reflected from the building and the tone-noise complex, under change, as the bus runs along. Here the kinship of living organism and what is beyond the boundaries of the body—a kinship sustained by all of the physical and the biological sciences—becomes effective. If certain characteristics and properties of 'objects' could not repeat themselves in energetic patterns of bodily function, it is difficult to see how perceiving could go on; go on, that is to say, without appeal to a magical mind or to a magical eye, ear, or other 'organ and avenue of sense.' No mental copy of the object: no object stalking up through the optic or acoustic nerve: only the perceiving of the building or bus as standing-there or as running-from-right-to-left-below.[7]

Both of the perceivings instanced are, of course, sophisticated. Once started upon the functions of perceiving, remembering, or emoving, the body has been shown to command all manner of short-cuts and special devices. The case would be simpler and clearer if we were to observe the young infant, where we should doubtless find perceiving initiated chiefly by way of the digestive sequence. Here we have both determination and pattern, and here we find stomach, lips, arms and hands, cord and brain, re-

[6] The reflective reader may be astonished to discover in philosophical and physiological writings on perception how naturally and almost inevitably the instances and the arguments are based on the eye and seeing. We shall presently find the same sort of special pleading when we come to emotion, where the physiologist easily thinks in terms of anger, fear, rage, and pain; *i.e.* of alleged emotions which nicely 'feature' those bodily excesses wherein the physiologist's current interest lies.

[7] The subject of mimetic duplication has been touched upon in the writer's *New Field*, (119-122, 159, 170 ff., 392). A running account of the bodily support of the apprehending functions is given in the same volume (Chap. iii) and the fundamental characteristics of all the functions in Chap. x (esp. 321-331).

ceptors, and much besides, providing the necessary conditions for the early perceiving of (say) warm-milk-oozing-in-from-soft-white-fragrant-expanse. The actual qualities of the object-as-perceived we can only vaguely and awkwardly describe for the perceiving child, because the whole range of conditions with him is scantier and less organized than any which we know in our own perceivings. We can, however, confidently limit his perceived objects to those characteristics which can be duplicated within his organism under such conditions as those of feeding. It seems a fair heuristic assumption, borne out and to be borne out by thousands of observations upon the infant and other animals, that the germ of psychological functioning lies in the post-natal digestive course, which seems to supply the first conditions requisite to these operations. This is the core of perceiving. The very first products may vaguely present some aspect of the body, but since many receptors are initial points for transforming energies from trans-bodily affairs, these would soon supply 'pattern' and rapidly extend the range and variety of perceptive products.[8] We must, however, exclude from the early beginnings all remembering, imagining, prophetic acting, emoving, comprehending, and the other functions, which come later and which presuppose both searching and perceiving. Likewise must we exclude everything like a spatial order in which objects are discretely arranged. The alleged 'blooming, buzzing confusion' in infantile life we can safely leave to the imagination of the reflective philosopher.

An excursion into developmental history has seemed to be warranted because it has directed us toward primal necessities in the organism which is, at the time of birth, 'unpsychological' and therefore with its entire way to make in the direction of our present interests.

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[8] The biological sciences, with the notable exception of physiology and embryology, emphasize the active relations of body to environment. The SR psychologies follow this biological trend. In the writer's opinion, however, psychology does better when it emulates the methodology and the point of view of physiology. When it does, it escapes the Condillacian prejudices that mind awaits a stirring up from the 'outside' senses and the Watsonian and Pavlovian prejudice that organic 'responses' must be due to 'outside' stimulation. The living organism is an active physical system which is, in large measure, independent of other physical systems. In the advancing independence of the higher animal forms lies the genius of mammalian (and especially human) living which gives to the psychologist his primary clue to description and understanding. The contention that the psychological activities chiefly owe their origin to systems and operations well hidden within the body accords with this principle of *relative independence of the living unit*. The complementary principle of *kinship of kind and of energies* as between the living and the non-living or cosmical systems, a principle already appealed to in these pages, then provides for all the 'outside' relations of the organism with its 'environment' to be found in physiology and in psychology. (Cf. Bentley, Environment and context, this JOURNAL, 39, 1927, 54-61.)

It is impossible, in this context, to follow through the entire period of psychological development and sophistication. We return, therefore, to adult man in order to indicate, by examples as simple and as representative as we can command, the essential tissues, organs, and systems which contribute, first to perceiving and then to two or three of the other primary functions.

In the earliest exercises of perceiving, as well as in the primitive forms of searching, we have found the body to be concerned at large, especially implicating those great systems which are accessory to the digesting functions. As we now advance to the more derived kinds of apprehending to be found in the older and riper organism, we discover that the central nervous system has taken over many offices previously shared with these other systems. So far, indeed, has this transfer gone that it has become habitual in psychology to look only to a small part of the great ramifying neural system to account for those 'mental phenomena' which the neurologist is wont to include under the phrase 'consciousness, emotion, and personality,'—a conglomerate grouping of the same order as 'landscape, planet, and radiation' or 'mushroom, plant, and chlorophyll.'

We shall do well, however, to keep our eye upon the body rather than upon the cerebral cortex alone as we hope to discover the support of those functions which are our primary concern. The strategic position of the central nervous system for a condensed and practically instantaneous representation of the entire body, together with energetic changes in the environmental range of the organism, has often been remarked. Afferent and efferent connections, incited by, and effective for, many and varied somatic and extra-somatic events, and leading to the set of central zones, nodes, or 'levels' already described, are the first line of strategy. The second line is established by the fact that the central neural structures are so constituted that they mimetically duplicate many of the activities which go on outside the brain. Thus we have reason to postulate energy-changes in the thalamus, cortex, and elsewhere, which are similar to the energy-changes at receptor and beyond, as well as the energy-changes in gland, muscle, and movement, on the other side.

We dwell upon the brain, therefore, not because we expect to find there the physical parallel to 'psychical' processes (as badly implied by the ancient phrase 'psycho-physical parallelism'), but because activities there epitomize the entire range of bodily and extra-bodily support to that segment of organic operation which we call the psychological segment. And since the entire brain is so tangled and detailed, we use such a simplifying and clarifying device as the handful of cardinal zones of Papez.

The accompanying schemata (Figs. 3 and 4) represent the most likely central epitome for primitive searching and for perceiving. The first figure (primitive searching) implies that, so far as the brain is concerned, (1) energy changes throughout the body, and especially pertaining to the phases of the digestive cycle, reach the hypothalamus (Hyp) through the visceral tracts (Vs), reach the thalamus (Th) through many afferent systems, *e.g.* the cutaneous (C); (2) proprioceptive avenues (Pp) affect thalamus (Th) and cerebellum (Cbl.); (3) the several offices of thalamus, hypothalamus, cerebellum and striatum (Str) are united by numerous connec-

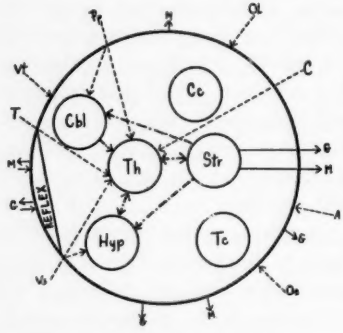


FIG. 3. PRIMITIVE SEARCHING

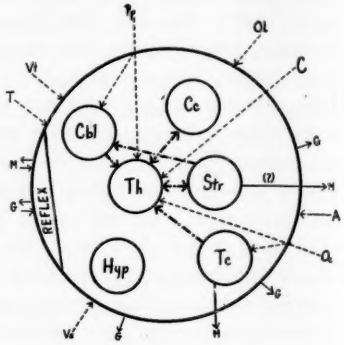


FIG. 4. PERCEIVING

tions, and (4) discharges to muscle (M) and gland (G) complete in principle the central representation for primitive searching. The cerebral cortex (Cc) may, to be sure, be involved; but there seems to be no primary dependence upon it. The schema fails naturally to show the characteristic phasic differences in pre-feeding, feeding, gastric digestion, and so on, and fails likewise to show the progressive changes from day to day and from week to week which give a unified and determined course to the entire cyclic series.

The schema for perceiving (Fig. 4) presents certain important modifications from the preceding. (1) Hypothalamus (Hyp) is not necessarily functional. (2) Proprioceptive impulses are represented as reaching cerebellum (Cbl) and thalamus (Th). (2) Thalamus is accessible from practically all receptorial sources (C and Oc are indicated in the figure). The ocular impulses (Oc) are shown as involving also the tectum (Tc) and commanding adjustive movements of head, eye-muscles, etc. (4) Reciprocal connections are indicated as between and among Th, Str, Cbl, and Cc. All

these express integrative activities, cerebellum characteristically yielding energy; thalamus re-sorting impulses bound for cortex and striatum; striatum preparing for appropriate motor discharges, and cortex patterning, reconstructing, orienting, specifying, and completing. These changes cannot be regarded as simply additive from the various parts of the cortex but as somehow representing a global activity which includes also thalamus, striatum, and perhaps some other central regions. (5) Of all the psychological functionings, determination is slightest in perceiving, involving, to some extent, the motor apparatus. Our present knowledge of the brain gives us very little specific information upon the high integrations required for even the simplest instances of perceiving. Moreover, the schema fails to do justice to the temporal running off of the function. Many experiments which have employed the method of fractionation have made it evident that perceiving is not a static and instantaneous ejection of a finished object, the building or the bus. Rather does the occurrence take its course and come to a natural completion.

*Acting.* We pass naturally from perceiving to acting, for an important functional relationship obtains between them. We must, at the outset, make a sharp distinction between the physiologist's movement and the psychologist's acting. Movement usually implies neuro-muscular devices, bony levers, ciliary beat, or some equivalent bodily mechanism which provides for locomotion or local change of position. Acting, on the other hand, is a function of fixed temporal phases which starts, in its simpler forms, with the apprehension of an object, then runs into a task (*e.g.* the lighted cigarette is to be taken from the table top, the telephone receiver is to be grasped and put to the ear), then the task is prophetically carried through, then the appropriate movement is completed and the effects of it noted. Both to the behaviorist and in everyday practice, movement and its achievements are more obvious than the psychological acting. It is only the psychologist, however, who has disentangled the complexities of the event just described and has given a practically complete account of the actional function.[9] Many thousands of observations, made under careful experimental conditions, have depicted this activity throughout its natural course, which may be written down, in paradigmatical short-hand as follows:

Apprehension of object-as-task-setting > anticipative realization > completion and realization through movement.

[9] Acting has a large number of variant but related forms. We confine ourselves here to a basal form, which is generally known in the laboratory as the *simple apprehensional* variety. A fuller account will be found in the author's *The New Field of Psychology* (chap. vi).

The actional form for which we seek bodily support may be represented by the instance of the cigarette or the telephone. Let us see how far the neurologist is able to help us.

Since the instances of acting which we have chosen as examples include perceiving, we may begin with the cerebral diagram for that operation. What now occurs which makes the seeing of building or hearing of bus essentially different from the grasping of the lighted cigarette? It is obviously the greater emphasis on *determination*, which prophetically anticipates the movement and its results. Real acting always has this anticipative 'to-be-done' running through it. It is, therefore, even more closely related to

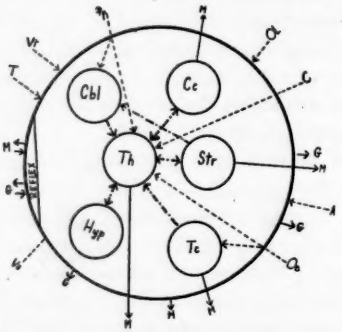


FIG. 5. ACTING: THE APPREHENSIONAL IMPULSE

that primitive form of imaginational apprehension which reaches ahead to the about-to-be than it is to perceiving. Indeed, we assume (cf. Fig. 1) that acting began with a functional union of primitive searching and a rudimentary anticipative apprehension. Were we to account in bodily terms for the *earliest* instances of acting, we should therefore combine our provisions for primitive search and the original kind of imagining. In our adult instances of building, bus, and cigarette, however, we are compelled to find more provision in the brain and less in the viscera. Here neurological ignorance checks us; but it is fair to conjecture that the visceral deficit for pushing on in function (determining) is made up from the cerebellar, thalamic, and cortical (probably precentral and frontal) reservoirs and also from striatal and tegmental discharge toward muscle (as through rubrospinal channels). The coloring which acting adds to (patterned) perceiving (that cigar is to be picked up) intimates that motor discharges from the cortex (pyramidal, extrapyramidal, thalamic, and others) occupy an impor-

tant place in this kind of functioning. By the same token, it is highly probable that the degenerate, foreshortened, or automatized actions involve the cortex less and less as habituation proceeds. The end of automization is, of course, no-prophesy, no-anticipation, and therefore no-acting. Incidentally, this relief leaves the cortex (and some other 'levels') free for other psychological sorts of activity. Violin-playing, painting, and sculpturing are good instances: little or no acting is required for the skilled and the gifted. These main facts and conjectures for bodily support of this function are tentatively set forth in Fig. 5. We must pass over all those interesting and important variants of acting that appear in choosing, selecting, rejecting, and resolving, as well as in the chaining of actions together in more comprehensive operations, and in the thinning out of them in the direction of automatization and temporal foreshortening. In all these forms our psychological knowledge far outruns our understanding of the nervous system and of the body at large.

*Comprehending.* Here the psychologist has not gone, in his descriptions, very much beyond the discovery of a closely knit group of operations and of their family relations to inspecting, perceiving, goal-searching, emoving, and the other modes. On the neural side, our knowledge is still scantier; much that the psychologist has in hand has not yet been used by the neurophysiologist.

Again we must be content with only one or two variants of comprehending and with only two or three of its family relations. Taken in gross terms, it is by comprehending that we get at the significance of things, events, written and spoken materials, and the character and intent of our fellowmen.

Take instances. The bridge ahead is gone: that signifies both 'slow down' and 'recent flood.' The cold is increasing: that signifies 'look to the radiator on the car.' The workman cries 'careful' or he waves toward you and points right: that signifies 'an explosion is imminent.' The waiter grabs my tip and grunts: that signifies 'displeasure.' It is obvious at once that comprehending has many occasions for its exercise and that, when widely used, it is designed to carry the organism far beyond the products of simple perceiving, remembering, imagining, and acting. A large part of what we call 'knowledge' is the product of comprehending. Let us inquire into its characteristics.

First, it is obvious that comprehending often includes perceiving, for it is frequently the product of perceiving that signifies, that has a meaning which goes beyond itself. The relation between the two functions is clarified by the intermediation of *inspecting,* which lies just to the left of comprehending in Fig. 1. Now inspecting is scrutiny, boring-into. When we in-

spect a product of perceiving (*e.g.* the roast of beef on the plate, the new gift, the face of the stranger), we amplify, analyze, distinguish, realize that product. Inspecting emphasizes the determined aspect of functioning, as does also primitive searching. In inspecting we never 'snap-shoot' an object or an event: we hold, deal with, and change it, usually on the side of enrichment. When we go a step farther, however, and comprehend, the enrichment of the inspected product fades and the thing signified appears instead. This process of comprehending may go so far (as in reading a detective story) that perception-products (the page, body-position) and inspection-products (the type-style, the striking of the clock) disappear and only the significance of what is read (the plot, the guilty person) is

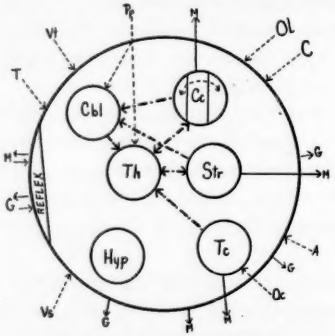


FIG. 6. COMPREHENDING

left. Here practically the whole organism is given over to signifying, *i.e.* to comprehending. As a rule, however, some of these other functions are beaten up into comprehending, so that we do know something about the bodily support when we have at least guessed at the organic foundations of perceiving, searching, and inspecting.

But just what is specifically added or changed in our central 'levels' or zones when comprehending goes on it is difficult to say. Fig. 6 suggests that we need here to introduce zonal differences *inside the cortex;* not indeed to put perceiving in one compartment and comprehending into another. The psychological descriptions do not point in that direction. They rather imply the integration of more areas or else a new accenting of areas already used in perceiving and inspecting. (The rendering of a sonata does not call for a special keyboard in the piano but rather for a new pattern of performance.) It is more than likely that long-continued and repeated functioning of the central nervous system (not memory, in the

sense of remembrance) is implied, though the neurologist has not given us an empirical account of the residues of brain function. The speculative 'trace' and 'engram' are useless. At any rate, there is nothing in comprehending itself which leads us to believe that a corner of the brain or a region of the cortex is uniquely called upon for this office. The figure intimates (without much neurological knowledge to go upon) that frontal, medial, and posterior parts of the cortex may be bound up in a particular way with cerebellum, thalamus, and striatum, and that afferent and efferent associates of these levels may play their respective parts as occasion demands.

*Emoving.* Physiologist, neurologist, and clinical student of the disorders (commonly called, in medicine, the 'mental diseases') have of late written voluminously on the cerebral basis of 'emotions.' They usually set out from an undefined and undescribed 'fear,' 'anger,' or 'rage,' and then proceed to the discussion of certain bodily changes—chiefly visceral and glandular—which they assume to be characteristic of the emotion alleged. These bodily changes are supposed to be symptomatized by movements and postures, by sweating, crying, and the like, which are then said to be 'expressive' of the emotion. The way seems thus prepared for examining the internal processes symptomized and for discovering their relations to autonomic and cerebrospinal structures.

It is unfortunate, both for neurology and for psychology, that the bodily states and changes mentioned have not been factually treated and described by the physiologist, but have rather been regarded as 'expressive' of something which is assumed to be, but is never really defined as, 'emotion.'

Hundreds of bodily poses, postures, movements, and changes of feature in the human animal are *socially significant to other men, i.e.* they call for comprehending. In themselves, however, they 'express' nothing. To the socialized observer they signify, they are symptomatic or indicative of, many things; of health, hesitation, fatigue, approval and disapproval, of intent to act in a specific way, and of many other activities to which the observed organism may be at the moment inclined. Among these many indicators we may—to be sure—read off what we regard as signs of fear, anger, hatred, and the like. These readings are, in the first place, extremely equivocal, so that when we separate them from other sources of knowledge (as in scrutinizing the isolated photograph of a face) we make the most egregious mistakes and confusions. In the second place, they can be relied upon at most to indicate an entire class of fears, angers, and the like, but can never be substituted for a first-hand account of a given and specific emotive functioning itself. When it comes to reading off an emotion from the gross behavior of a dog or cat strapped to an operating table or from the violence of the caged or captured wild beast, the 'expressive method' is little better than the loose arts of divination. The outcome confirms this verdict, for a large number of divining names used (*e.g.* feeling, affection, excitement, affect, aggressive activity) can be brought by no descriptive method under a common class to be sensibly called 'emotions.'

The emphasis recently laid upon autonomic and associated chemical processes and their central correlates has focused attention upon the hypothalamus as an alleged 'seat' of the emotions.[10]

The psychologist who begins with a specific and describable operation of the organism, which he names 'emoving,' is bound to ask, first, whether the undefined 'fear,' 'anger,' or 'excitement' of the physiologist is (in the cases cited by him) a real instance of emoving; secondly, whether the cases cited are representative; and, thirdly, whether the bodily postures and movements interpreted as 'expressive' of emotions are specifically indicative of such a class of internal changes. Unless and until the psychologist is persuaded upon these matters of high assumption and speculation, he is not likely to be greatly moved by the current theories of a sub-cortical 'seat' which are based upon them. He may well find each of these assumptions unsupported by fact and untested as theory. His task would be much easier if the physiologist were to state, *in anatomical and physiological terms,* the results of his observations upon autonomic tracts and connections, upon visceral changes and glandular secretions, and upon cortical and subcortical connections and processes. The psychologist would then learn of many important discoveries regarding neural tracts, nuclei and connections, and regarding the functional relations of these to the organs and processes of the big body-cavities, and to skin, muscle, and gland; but he would be cautious about the medley of physiological fact, dubious psychology, and unrestrained speculation which has led to the claim that the posterior hypothalmus is not only responsible for emotion but is the source of things so diverse as impulsive activity, dynamics associated with sensations, perceptions, and attention, and with motor and ideative impulses; that injury here produces mental fatigue, emotional indifference, troubles of receptivity and orientation, manic and schizophrenic states, alterations of mood, of sentiment and personality, stupidity, apathy, somnolence, lethargy, and mental deterioration.[11] If the psychologist did meet these astounding claims —actually made of late for the subcortical region in question—he would be inclined to make the same allowance as for the excited survivor of an air crash who might declare that a cracked propeller wrecking the plane was the seat of bone breaking, nose smashing, leg crushing, engine burying, money losing, telegraphing, grieving, loss of consciousness, dying, burying, company suing, and all the other sequels to a fatal accident.

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[10] It may be that these physiologists do not realize the influence exerted by the Freudian 'unconscious' upon their subcortical soundings and the theories therefrom.

[11] This medley of things centered in a hypothalamic area has been collected by Papez, Cerebral mechanisms, 1937 (in press).

When we start with the emoving itself, deferring all theory until we have the necessary facts, we seek first the place of emoving in the chart of psychological functions. We find it in the executive group, closest of kin to acting (Fig. 1). Once we know how the prophetic and goal-reaching-by-way-of-organic-movement mode of acting is carried out by the body, we have the first clue to emoving. Although actions are of many grades and forms, we have found that our bodily knowledge is fairly adequate to the simplest forms of this function. Let us start here and seek the bodily means for that typical modification of acting which is emoving.

Now the distinctive feature of the form of acting which stands closest to emoving is that its first stage or phase presents a significant scene or event; *e.g.* I comprehend the significance of my brief telegram and *then* prophetically seek the appropriate goal. With this kind of acting, it is only necessary that the product of comprehending becomes a *predicament;* the telegram requires that I go to my sick child but floods have washed away the roads and I cannot leave, a rival firm threatens my business and I am without money or friends. That is to say that an actional goal is set but cannot be attained. The significant situation seizes me and I cannot escape. Some critical affair demands that the body be active toward an accomplishment, while circumstances prevent the natural completion of the action.

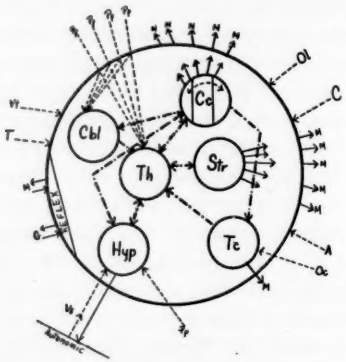
In order to understand how the body emoves, then, it will be necessary (1) to know what happens when the determined course of acting miscarries in such instances as those just now cited, and (2) to review the body while it is comprehending. We should find that the bodily devices for emoving spring from an integration of these two operations and from the resultant issue of their union.

As we have seen, the neurologist has given us very little information about the bodily means for comprehending. If the form of cortico-thalmic-cerebellar-striatal organization suggested above for this function is specifically responsible here, we are prepared to ask what is added to turn the acting-comprehending into emoving and thus to precipitate tentative and unsuccessful attempts to flee, strike, insult, menace, defy, avoid, or otherwise to remove the predicamentive character of the significant scene or situation.

Here we must distinguish between that comprehended situation which defeats the organism and that which does not. Such street signs as 'slow down,' 'detour,' and 'turn right,' are also comprehended: they are taken as significant: they mean a situation calling for active performance. But they are imbedded in an action which runs its course and reaches its proper goal by way of turning the steering-wheel. Where the situation is predicamentive, on the other hand, the action does not come off. It is blocked. For the moment, at least, the psychological functions fail to

reach the actional goal; bodily and extra-bodily changes which might resolve the predicament are wanting.

Let us turn back to our tentative neural schema for acting (Fig. 5). Here we find a high functional integration of cortex, thalamus, and striatum, with incoming proprioceptive impulses and outgoing motor discharge to muscles. Now (for emoving) add, on the one side, anarchy and failure of all outgoing discharges (ineffectual movements) with a piling up of proprioceptive returns from every part of the moving apparatus; and add, on the other side, impulses from the integrated ('comprehending') cortex to thalamus and hypothalamus (possibly also to striatum, tectum, and cerebel-


Cb
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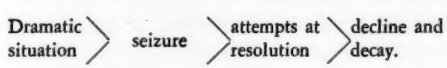
cortex or thalamus. Again, the bodily patterns for comprehending and emoving (2) show distinct similarities with respect to the cortical involvement. Hereupon rests the 'situational significance' basal to both functions. But in emoving an incoördination as between thalamic-cortico-cerebro-spinal discharges, on the one side, and, on the other, hypothalamic complications with the cortex and with reception from viscera, muscles, and (probably) thalamus change the central organization and lead to the distinctive coloring of emotive functions. The secondary implication of automatic and viscera agrees with the psychologist's descriptive account, which makes the somatic flavor of the emotive product *accessory*, not primary, to the main course of the function,[12] which runs




This is the basal formula for emoving. It indicates that the function has characteristic phases and a characteristic temporal order. The physiologist's exaggeration of visceral and autonomic factors would seem to be explained by the fact that he combines a current interest in certain bodily organs and in the part which their operations play in life with an obscure and vascillating conception of what an 'emotion' may be. His very great advances in our knowledge of certain subcortical regions and the relation of these to the physiology of the trunk would scarcely lose in significance were he to omit the term 'emotion' altogether and control his desire to speak the uncertain language of 'consciousness,' 'psychisms,' 'feelings,' and 'affects.'

Thus it appears that the physiologist, setting out from many careful and fruitful studies of visceral and autonomic functions, has come more or less by chance upon the problem of a bodily 'seat of emotion.' Tracing the neural initiation and control of his great internal operations to thalamus and hypothalamus, the physiologist has been tempted to substitute biological speculations about the 'expression' of the emotions for a factual description of emoving itself. Moreover, the central fact of predicament and the significant relations of emoving to acting and to comprehending have escaped him. The natural consequence is that his theory of a 'center' for emotion accounts only for the adventitious and secondary fact that emoving sometimes precipitates accessory functions of gland, viscera, autonomic, circulatory system, and other somatic structures. The psychologist's direct approach to the body from the central facts of emoving yields quite a different perspec-

[12] *New Field*, 245. Several modifications of the basal formula will be found on pp. 247-252 of that work.

tive and notably shifts the view of neural integration underlying this great and primary class of psychological facts. The entire complexion of the problem, again, is radically changed by eliminating the magical notion of 'something psychical' *as existing within the brain* and as requiring a residential seat there.

All this discussion of the way in which the body does its emoving, its comprehending, its searching, and the like, is very sketchy and wholly preliminary—preliminary in spite of milleniums of talking and writing upon such problems as 'Why the mind has a body,' 'Whence the body gets its mind,' 'How the physical and the mental can preserve an alleged parallelism,' 'In what part of the brain consciousness, emotion, and personality reside,' and many others of the same tenor.

The account is sketchy from the obvious limitations of time and space, and sketchy because little is known of those conditions of bodily structure and operation which can be turned to account for the benefit of psychological fact. It is preliminary in the sense that once the validity of our problem is recognized, the quest of bodily support is bound to take the psychologist much farther than the preceding pages have indicated. Let us ask 'how much farther?'

In order to avoid a misreading of the figures, which are designed to indicate in a wholly tentative way the engagement of the body in the several psychological modes of functioning, the reader should clearly understand that the neural diagrams (Figs. 3-7) represent only a single, simple form of each mode and as limited an involvement of the body as can well be represented. Unless he is well acquainted with the many variant forms of (say) perceiving or emoving, it is altogether likely that his own instances, chosen at random, will fail to fit in all details the schemata of the text. Of course, like variations appear also in the digestive and other functions described by the physiologist. Again, it is important that the reader know that it is the exception, not the rule, for a single, primary function to run its isolated course save under experimental conditions. In the common activities of man, and of many other animals too, several of the primary functions usually course concomitantly in integrated and derivative form. Here as elsewhere functional analysis implies abstraction just as much as anatomical or morphological analysis does. It may be unnecessary to point out that the 'blank' areas within the large circle (which limits the central nervous system) are also active and are, indeed, a necessary part of the functional patterns of the brain. Their inclusion, as well as any attempt to represent in a graphic manner the temporal course and the phasic changes of bodily activity, would have imported hopeless complications—as well as troublesome neurological uncertainties—into our difficult problem.

Now we must look more generally than we have over the wide field which the psychologist has undertaken to cultivate that we may examine large salient areas and subdivisions. This new inquiry is made in the interest of

securing for a wider field a sound physiology to sustain a sound psychology. We shall be content with fair instances and illustrations. Our first subject is learning.

Psychological accounts of *learning* have usually spun a speculative web of physiological fancy to make plausible the facts of acquisition, habit, and skill. The results have been a formal and barren neurology of the establishment of associations, of habit formation, and of the dexterities. Learning, in the sense of educational promotion, has usually taken the place of real psychological change, and educators have lent dignity to this speculative neurology by calling their dogmas 'laws.' The common method has rested upon an increase of *product* (more associations formed, better habits established, and new dexterities acquired). Omitting a factual account of the *actual processes of change* ('the learning process' is usually a psychological caricature), men have jumped from amount of product straight to such things as 'brain traces,' 'engrams,' 'associational centers and pathways,' 'bonds,' 'neural channeling,' 'synaptic resistance,' and the like, all terms which seem to have borrowed respectability from serious use by physiologists and biologists.

Now it scarcely seems likely that we shall acquire a substantial understanding of the bodily devices for such products as these until we can describe *what is actually going on* as the organism learns. Thus we must look for changes and modifications in course as the psychological functions proceed under exercise, moment by moment, day by day, and month by month. With these functional modifications known, we may then go to the body to ask it the familiar question 'how?'

We now know that some of these characteristic functional changes are by way of addition, elimination, substitution, consolidation, and stabilization, changes which are under close scrutiny in the laboratory. Once we discover bodily ground-work of the functions and of these modifications of them, we shall have a real understanding of what learning is and how it proceeds and produces. Here the psychologist makes avid use of new facts in neurology and general physiology, in the 'conditioning' experiment (doctrine apart), in pathology, and elsewhere.

A wide opportunity for improvement appears in the human *genetic* field. Here our primary aim is a running account, written in psychological terms, of the entire life-course from fertilization to age—not merely of those two decades wherein parental and public responsibility exaggerate the immature years as against the others. An adequate genetic psychology, divorced from the concept of economic, family, educational and social *perfecting* of the young by remodelling them into adults, will make many

demands upon physiology, heavier demands than that subject is now prepared to meet.[13]

As a by-product, such a developmental psychology and its included account of bodily change should render unnecessary (save to the romantic amateur and the lover of psychical mysteries) such a misleading concept as the Unconscious, with all its alleged 'mechanisms.'

Among the *other special psychologies* (animal, social, anthropological, individual, and abnormal) we decidedly need to know how precisely the body brings about the things which we observe in these several fields. Under the combined spell of education, biology, and medicine, some psychologists agreed to close their eyes to 'mental' facts and to proceed as if only agencies in the environment (conveniently called 'stimuli') moved the organism. Thus did the personal, the social, and the biographical life of man disappear by a wave of the behaviorist's wand. Disappeared also most of the relevant problems of physiology, for the body was but a pliant instrument under the sway of external forces. With the return of sober and more responsible views in our genetic, social, and comparative psychologies, there is less temptation to shirk the responsibility of finding just how (by what bodily means) the personal, the social, and the biographical—as well as the strictly behavioral—appear in the life of man and of the other animals.

The demand is startlingly realistic in the realm of the *disordered and the defective*. Every year it becomes more apparent that the facts of inadequate, disturbed, and disordered living call for a new setting and a new orientation among the natural sciences as well as among the arts of practice. Every one who objectively observes these facts may now discover that the relegation of them, as mental diseases, to medicine rests upon an adventitious fact of history. The superstitious views of madness and lunacy called for responsible interference. Medicine generally extended its domain a century ago to cover the pathology of the mind. This was an humanitarian *tour de force*. The concept of 'mental' disease has never been logically justified, while the newer concept of pathogenesis has, within the last half-century, made medicine increasingly apologetic and defensive for its adopted child. On the other side, psychological enlightenment upon alleged mental agents and forces and the observation of social factors in disorder have helped to render the notion of a mental variety of disease both incongruous and unnecessary. Inertia, folk-beliefs, medical education, and the unwillingness of a profes-

[13] It can hardly be assumed with safety that a single functional plan of the brain will serve from birth to age. A given psychological function may well be supported quite differently at different periods of life.

sion to relinquish practice and to limit social and political prestige still tend toward an equation of disorder with disease. Large expenditures for housing and care, still chiefly in the hands of medical institutions and organizations, also cloud the entire problem. Presently a new partition of responsibilities among the sciences of life, the institutions of the state, social studies, and the natural means of care and relief, will be made. Then psychology will discover that it has done its part—the description and understanding of disordered and inadequate living—very badly. It will need to improve its account of functions, adequate and disturbed, of government and misgovernment of the organism, and of the effect of unwholesome psychological products (notably products of emoving, imagining and impaired comprehending) upon sane living. In no group of problems will psychology make greater demands upon a competent neutral somatology, in order to derive the bodily foundations of unsane and insane living. Here pathological anatomy and diseased tissues will demand careful consideration, wherever they are discoverable; but all the other relevant facts of body, of the physical world, and of the social ordering of man's life will receive the same methodical attention. It may be that the concepts of 'mental disease,' 'mental medicine,' and 'psychogenic disorder' will then have only an historical significance or, at most, an institutional and doctrinal sanction. At all events, the general clarification of the problem of deranged and inadequate living should rid us of the present abundant crop of books upon this subject which bear the title, 'psychiatry,' 'psychology,' 'mental hygiene,' and 'psycho-analysis' but are nevertheless hopeless medleys in which all these subjects are interfused without logic or order.

Our attempt to discover how in principle the living body conducts its psychological affairs confirms the initial suspicion that ignorance here far outweighs knowledge. Doubtless inherent difficulties of the problem are in part responsible; but neither psychology nor physiology is to be held blameless. Psychology has been distracted by too many theories, too many partisan rivalries, and too many premature deflections into the clamoring arts of practice. It has, moreover, been content to build into its own structure deadwood and rubbish from outworn physiological theories. Physiology has seldom made a business of acquiring its psychological facts at first-hand and of using them coherently. Nor has it generally been content to limit its descriptions to the bodily facts of organ, tissue, cell, and function. Too often has it filled its gaps with psychological padding, and too often this padding has been worthless or positively destructive, reflecting an uncritical acceptance of outworn doctrine, speculative theory, and unattested fact.

I have not meant to imply that the psychologist who regards his task as

something more than an apprenticeship to the biologist or the neurologist should set out from a particular system of psychological concepts or terms; but only that, if he is to relate his facts to the sciences descriptive of the body, he should have his facts in order, make his principles coherent, and see that his alliance with the other disciplines of life is both significant and fruitful.

# AFTER-IMAGES OBSERVED IN COMPLETE DARKNESS

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Although the latter and major portion of this paper is concerned with the flight of colors in the after-image observed in complete darkness, this problem is fittingly introduced by showing the relation of after-images observed in complete darkness to those projected upon a bright homogeneous field. The latter type of after-image depends upon the diminished excitability of a stimulated area to fresh stimuli. For example, if one looks at a bright object and then fixates a large bright field, the retinal area corresponding to the first stimulus, since it has been fatigued, gives an image darker than its surroundings. We shall refer to this type of after-image as a fatigue image in order to differentiate it from those observed in complete darkness.

Fatigue images may be supposed to result from two causes: (1) fatigue at the synapses in the retino-cortical pathways; and (2) depletion of excitable substance in the photoreceptors. If synapses play a rôle, the ones involved must occur at some point below the convergence of the paths of the two eyes, for, if the retina to which the primary stimulus has been applied is faintly illuminated by letting light fall on the closed eyelid, a negative fatigue image appears, whereas it fails to appear if the opposite eye is similarly illuminated.

The after-images observed in complete darkness seem to arise from persistence of excitation after the withdrawal of the stimulus and will be referred to in this paper as persistence images. They are usually positive,[1] in the sense that when the primary image is a bright object in a dark field, the after-image also is seen as a bright object in a dark field. Under certain conditions the image observed in complete darkness is negative, *i.e.* it in-

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[1] Although the terms *positive* and *negative* are often used to designate same or reversed brightness relationships, many writers use the term *positive* to apply to after-images of the same hue as the primary stimulus, and *negative* to complementarily colored after-images. This double usage is confusing because often an after-image may be positive with respect to brightness and negative with respect to hue. For this reason certain writers have applied *positive* and *negative* to brightness only and have used *same colored* and *complementarily colored* for hue. *Positive* and *negative* have also been used to differentiate after-images projected on a bright field (fatigue image) from those observed in complete darkness (persistence image). The justification of this use is that fatigue images show a reversal of brightness relationships from the primary stimulus, while the persistence image maintains the same brightness relationship; but this distinction is not strictly true for in the latter case the brightness relationships may be the same or reversed.

volves a reversal of brightness relationships. This is especially true in the last stages of the after-image. When the intensity is low, the negative stage is the only effect to be observed, and the initial positive stage is absent altogether.

Several theories have been advanced to explain the negative image perceived in darkness. Helmholtz attributed it to the diminution of the intrinsic light in the stimulated area of the retina.[2] The negative after-image observed in complete darkness occurs chiefly with weak stimuli, where the persistence effect is absent and the intrinsic light is the principal factor determining the appearance of the visual field. Helmholtz assumed that whatever causes the intrinsic light is inhibited by previous stimulation of the retina. Hering proposed a theory of reversible processes in which a catabolic process operates while the light acts upon the retina and gives a bright sensation.[3] This is replaced by anabolic processes when light is withdrawn and the black sensation results. McDougall pointed out that Hering's theory is inadequate to account for the bright positive after-image obtained with stronger stimuli.[4] McDougall used a graded stimulus with a large area and was not able to obtain a negative at all regardless of the intensity employed.[5]

The persistence image arises either from a persistence of activity in the photoreceptors or from a persistence of activity at the synapses. There are several types of evidence relative to the seat of the persisting activity. One fact which tends to localize the persistence effect in the photoreceptors is that the fovea fails to give a positive persistence image with a small area, whereas the periphery does. When a circular stimulus subtending a visual angle of 1°, was used with foveal fixation and with a fixation-period of 20 sec., neither of the authors obtained a positive after-image; but with peripheral fixation 10° from the fovea only a positive after-image was obtained. The phenomena just described can be explained by assuming that the persistence effect is more pronounced in the case of the rods than in the case of the cones.

Hartline and Graham,[6] in contradiction to the idea that the persistence effect is seated in the photoreceptors, have demonstrated that the photoreceptors in the eye of Limulus show no after-discharge, and, if this is true of the human photoreceptors, the persistence image must be attributed to synaptic after-discharge. Adrian and Matthews have demonstrated that the optic nerve of an eel, which is preceded by synapses, shows a prolonged after-discharge.[7] That the principal seat of the persistence effect in the human subject lies at some point prior to the convergence of the paths from the two eyes is demonstrated by the following experiment. When the eye is left in darkness, the persistence effect dies away in about 2 min. with even the

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[2] H. von Helmholtz, *Physiological Optics*, II, 1925, 266.
[3] E. Hering, *Wissenschaftliche Abhandlungen*, 1, Chap. 39.
[4] W. McDougall, Some new observations in support of Thomas Young's theory of light and colour-vision, *Mind*, N.S. 10, 1901, 52-97, 210-245, 347-382, esp. 52 ff.
[5] McDougall, *ibid.*, 210-245.
[6] H. K. Hartline and C. H. Graham, Nerve impulses from single receptors in the eye, *J. Cell. & Comp. Physiol.*, 1, 1932, 277-295.
[7] E. D. Adrian and R. Matthews. The action of light on the eye: I. Discharge of impulses in the optic nerve and its relation to the electric charges in the retina, *J. Physiol.*, 63, 1927, 378-414.

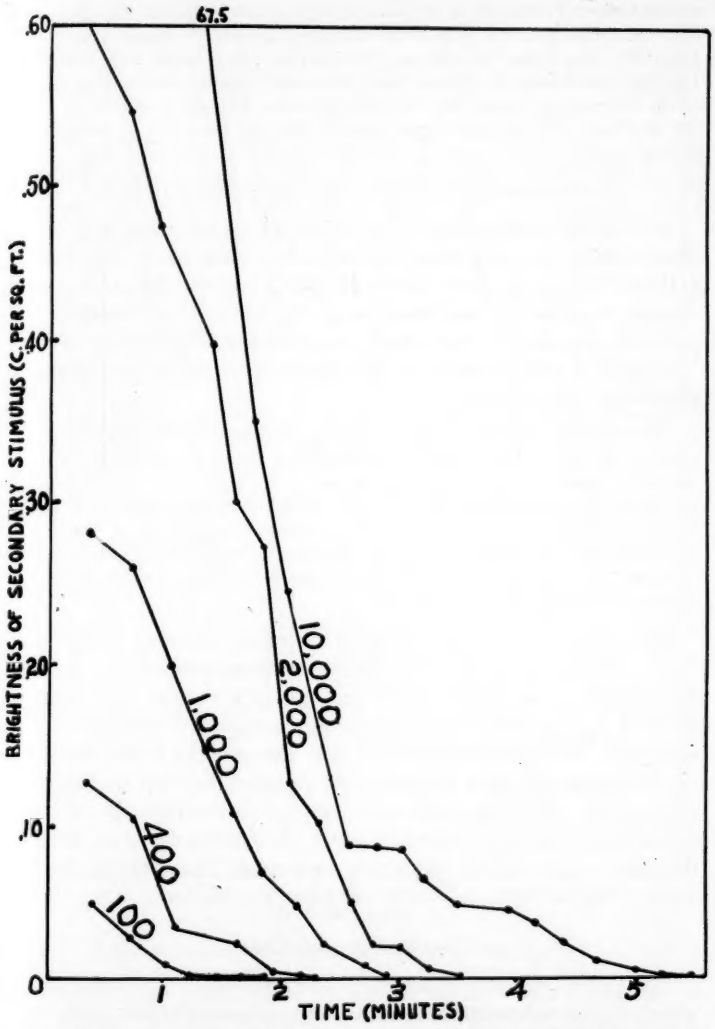


FIG. 1. BRIGHTNESS OF THE FIELD WHICH GIVES A BALANCE BETWEEN POSITIVE AND NEGATIVE AFTER-IMAGES AT SUCCESSIVE MOMENTS AFTER THE CESSATION OF THE PRIMARY STIMULUS

The different curves represent the effects obtained with different intensities of the primary stimulus, viz., at 10,000, 2,000, 1,000, 400, and 100 c. per sq. ft.

strongest stimulus; but activity essential to the image can be revived for a period of from 3 to 5 min. after this if the retina is faintly illuminated by exposing the closed eyelid to ordinary room illumination. However, the image cannot be similarly revived by illuminating the opposite eye. McDougall[8] reported that he was able to revive the image by illuminating the opposite retina, although it was not as effective as illuminating the same retina, but we have not been able to confirm this finding.

## Transition from Persistence to Fatigue Images

There is no absolute demarcation between a fatigue image and a persistence image, for the intensity of the surface upon which the image is projected may be gradually decreased until a negative fatigue image is changed to a positive persistence image. At the point of transition the persistence and fatigue effects cancel each other and no after-image is seen.

In order to obtain quantitative data for this phenomenon the following experiment was performed.

The *O* fixated a circular stimulus, subtending a visual angle of 6° 17′, with the right eye for 20 sec. At the cessation of the primary stimulus, the *O* fixated a small dot in the center of a sheet of milk glass, size 16 in. by 20 in., placed 16 in. from the eye and illuminated from the rear. The *O* then varied the illumination of the field until a balance was reached between the positive and negative images. As soon as the *E* could take the voltage readings the illumination was again adjusted by the *O*. This procedure was repeated until the after-image was no longer visible. Meanwhile the left eye was kept closed and covered.

The results for various intensities of the primary stimulus are shown in Fig. 1. The interpretation of the balance between positive and negative after-images is complicated by the fact that, if a positive after-image is observed in complete darkness, it gradually decreases in brilliance until it disappears, but it can be revived for some time after by faintly illuminating the retina. We have to distinguish, therefore, between the brilliance of a positive after-image observed in complete darkness and the brilliance which it can be made to assume by faintly illuminating the retina. Since in the above experiment the retina was continuously illuminated it must be assumed that the persistence factor was maximally effective.

## The Flight of Colors

*Methods of graphic representation.* The after-image of a white object, observed in complete darkness, undergoes a succession of changes in hue. In general, the sequence of color follows the psychological color circle[9] in

[8] McDougall, *op. cit.*, 217-219.
[9] C. S. Myers, *Textbook of Experimental Psychology*, 1922, Plate I, Fig. B, 89.

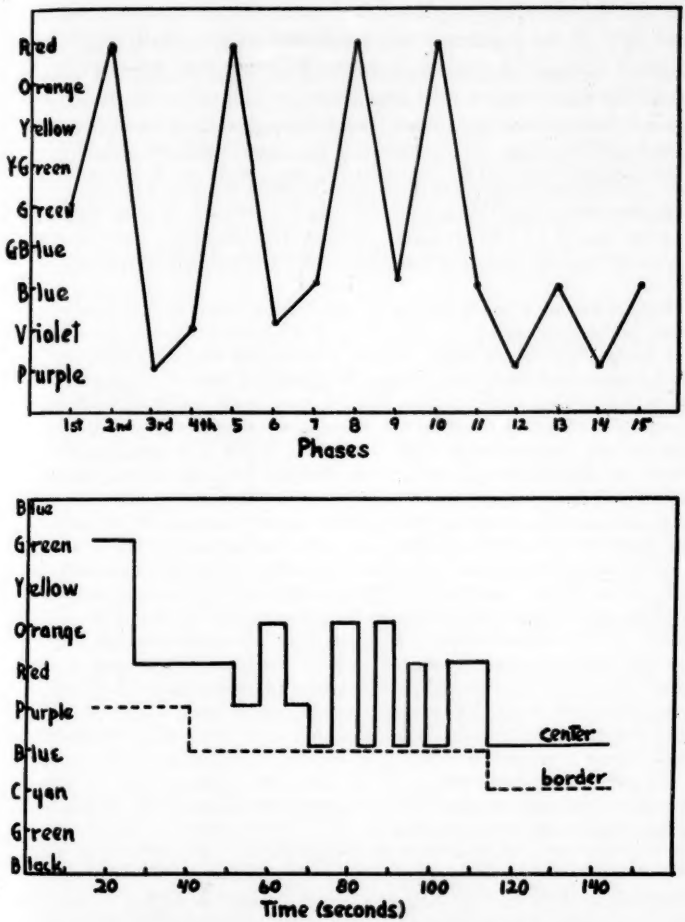


FIG. 2. FLIGHT OF COLORS AS A FUNCTION OF METHOD

The upper graph shows the flight of colors plotted according to Berry's technique, *i.e.* with the colors plotted on the ordinates according to their position in the spectrum, and with the successive phases of the after-image plotted along the abscissae. The lower graph shows the same data with the colors plotted as they occur in the typical after-image along the ordinates. The abscissae represent time after cessation of the stimulus. The upper graph shows only the color at the center of the image, while the lower graph the color both at the center and at the border. The stimulus for this experiment was a disk subtending a visual angle of 1° 55′ of a brightness of 2000 c. per sq. ft., and fixated for 20 sec.

the anticlockwise direction. The initial color depends upon the stimulus-conditions. If the stimulus is very bright, the initial color is blue, and is followed in order by green, yellow, orange, red, purple, blue, green, and black. The after-image usually turns negative with either the black or the green. Under certain conditions the oncoming color is seen first at the border and gradually encroaches upon the center. This phenomenon has been commonly referred to as involution. Under other conditions the change from one color to another is uniform over the whole surface of the image, and this change may be gradual or sudden. The after-image may disappear for short intervals and may reappear in the same or a different color.

Berry concluded from his findings that the flight of colors does not undergo the steady progression around the color circle that other investigators have found.[10] Two factors make his data appear to be inconsistent with what is regarded as typical for the flight of colors. In the first place, he plotted the colors in his graphs according to their position in the spectrum (see the upper graph in Fig. 2) and this misrepresents the flight of colors because it makes the transition from red to purple (see the jump from the second to the third phase) appear as a reversal in the direction of the flight, when as a matter of fact it represents a continuous transition in the anticlockwise direction around the color-circle. When the results in the upper graph are plotted as in the lower graph with the colors arranged in the order that they appear in the typical after-image, the artifactual reversals are eliminated.

In the second place, he has not allowed for the fact that he used a stimulus which gives the involution phenomenon. In the transition from one color to another, the oncoming color appears first at the border and encroaches on the center, and the spot of color at the center, under certain conditions, entirely vanishes and reappears, and may repeat this procedure several times. An instance of this is found in Fig. 2, where the color at the center of the image alternated between blue and red and later between blue and purple. This struggle is not inconsistent with the general tendency of the hue to undergo a gradual progression around the color circle, but it makes the graph appear as an oscillation rather than a steady progression. Under conditions which give an after-image without an involution, the results are easier to plot and the steady progression of colors is more apparent. The results of the observation of such a stimulus are recorded in Fig. 3.

*Variations in the flight of colors.* Berry's history of flight-of-color in vestigations show that investigators have obtained a variety of effects.[11] The discrepancies probably are due to the differences in stimulus-conditions and might be reconciled if the stimulus-conditions were known, but few observers have specified their conditions adequately enough to justify an

[10] W. Berry, Color sequence in the after-image of white light, this JOURNAL, 38, 1927, 584-596.

[11] Berry, The flight of colors in the after-image of white light, *Psychol. Bull.*, 19, 1922, 307-337.

attempt at reconciling their results. When stimulus-conditions are kept constant, different *S*s get essentially the same results.

Barry and Bousfield[12] used 24 *O*s and obtained what they regarded as a typical flight of colors with very little variation between *O*s. Berry seems to be the only investigator who has reported radically different results with different *O*s under the same stimulus-conditions. He not only alleged that details in the sequence of colors vary from one *O* to another, but also reported similar variations for separate observations with the same *O*. He concluded that a statistical treatment of a large number of observations with a group of *O*s is necessary in order to deal adequately

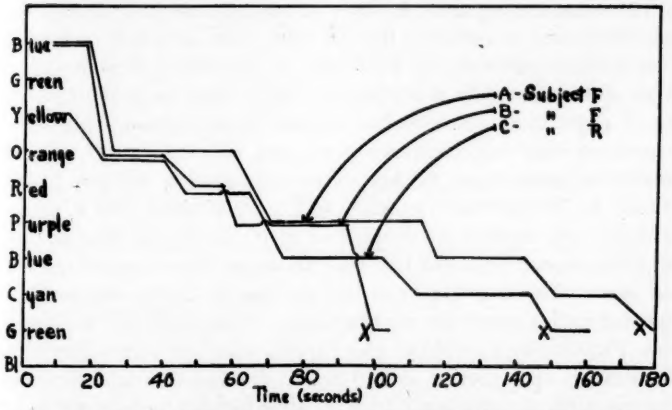


FIG. 3. SIMILARITY IN THE FLIGHT OF COLORS BETWEEN DIFFERENT *O*s AND THE EFFECT OF ONE OBSERVATION UPON AN IMMEDIATELY FOLLOWING ONE.
The point at which the after-image turns negative is indicated by X. The stimulus was a disk subtending a visual angle of 6° and having a brightness of 400 c. per sq. ft. and fixated for 20 sec.

with the facts. Berry's results, however, do not prove that a statistical procedure is necessary because his methods of treating his data are faulty and the very use of these methods makes the results of separate observations appear discrepant when in fact they are not fundamentally so. He has used an inadequate basis for comparing the results of separate observations. He has divided each observation into three parts and measured the dominance of each color in each part by the number of its appearances. The number of appearances is not an adequate measure of the dominance of a given color for the after-image may rapidly disappear and reappear in the same color, thus accentuating the number of appearances, whereas the total duration may be small compared with other colors which show fewer appearances.

He has also used the total number of phases as a basis for dividing the observa-

[12] H. Barry, Jr., and W. A. Bousfield, Implications of flight of colors, *Psychol. Rev.*, 41, 1934, 300.

tion into three parts. This is objectionable because the stage of the after-image in which a given color predominates is determined principally by the time after the cessation of the primary stimulus. If in a given experiment the after-image is prematurely cut off, as sometimes happens, it would not be fair to compare the final color in such an experiment with that in another experiment in which the after-image goes to completion. Also there might be a great number of fluctuations at the first part of one observation and at the end of another, and a division on the basis of the number of phases would not provide a fair basis for correlating the colors in the two observations. When a method like this is used for comparing the results of different observations the results can hardly be expected to be consistent.

The essential thing about the flight of colors that remains constant from one observation to another is that the colors come according to a definite time schedule following the withdrawal of the primary stimulus. In Fig. 3 are plotted the results of two separate observations for *F* (Fry) and one for *R* (Robertson), the stimulus-conditions being constant. That there is consistency from one observation to the next with the same *O* is demonstrated by curves A and B. These curves closely follow the same path for 90 sec. At this point the after-image, in the case of observation B, changed over to a dark negative and disappeared at 109 sec. This observation differs from observation A in that here the after-image turns negative and is cut off sooner. This does not mean that the changes in the after-image are speeded up, but merely that the latter stages of the after-image are dropped out. This shorting is typical of what happens when one observation follows immediately upon another, as in this case. The fact that different *O*s may get essentially the same result from the same stimulus, with respect to both time and color sequence, is illustrated by curves A and C.

*Effects of duration.* That the sequence varies with the duration of exposure of the primary stimulus has been reported by Helmholtz, Washburn, and others. Short stimuli are characterized by a rapid flight of colors, beginning with green and passing around the color circle in the clockwise direction to blue, where the flight slows up, reverses and proceeds in the anticlockwise direction. With long stimuli the clockwise flight is absent.

The following experiment was carried out to ascertain the nature of the transition from short to long stimuli. The exposure time was varied through the following steps: 1, 2, 3, 5, 7.5, 40, and 60 sec. The diameter of the disk-shaped test object was kept constant at a 6° visual angle and the brightness at 2,000 c. per sq. ft. The preliminary clockwise flight is obtained with the 1- and 2-sec. stimuli but not with the 7-sec. stimulus (see upper graph in Fig. 4). The preliminary flight presents a special problem and is not encountered as long as one deals with stimuli of over seven seconds duration.

The results of stimuli lasting 10, 40, 120, and 180 sec. are plotted in the lower graph of Fig. 4. In this experiment the brightness of the stimulus was 400 c. per sq. ft.; its size was 6° visual angle. In general there is a

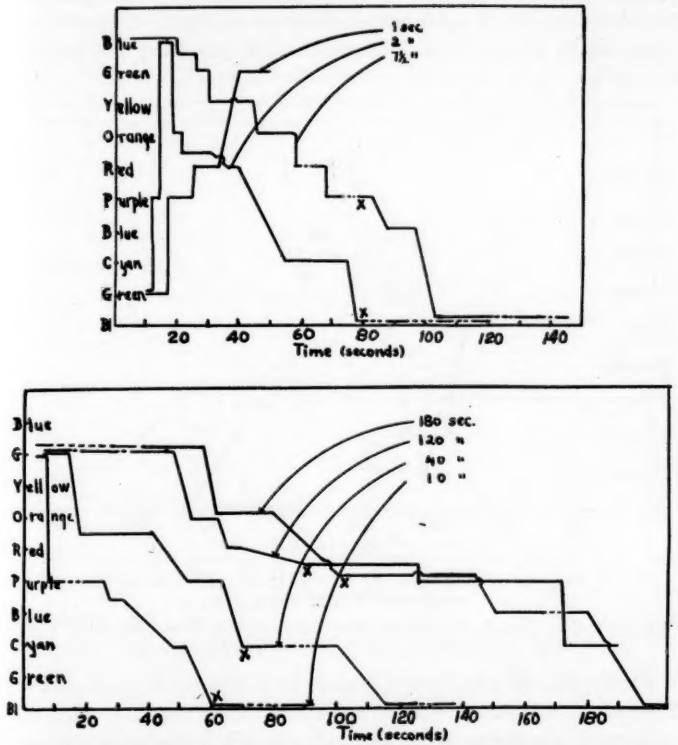


FIG. 4. FLIGHT OF COLORS AS A FUNCTION OF THE DURATION OF THE PRIMARY STIMULUS

The upper graph shows the flight of colors with primary stimuli of short duration. The lower graph shows the flight of colors with primary stimuli of long duration. The point at which the after-image turns negative is indicated by X.

tendency for the duration of the after-image to increase with the time of stimulation; each stage of the progression is delayed.

*Effects of intensity.* In the following experiment, *O* fixated for 20 sec. a stimulus which subtended a visual angle of 6°. Intensity was varied through 100, 200, 400, 1,000, 5,000, and 10,000 c. per sq. ft. An increase

in intensity is found to be accompanied by an increase in the duration of the after-image. With lower intensities, the initial stages are cut off, that is to say, the sequence begins with the second blue and, as the intensity is increased, the initial color tends to come from a more clockwise position in the color circle. These relationships are shown in Fig. 5. Others have reported similar results, in particular McDougall, and Barry and Bousfield.

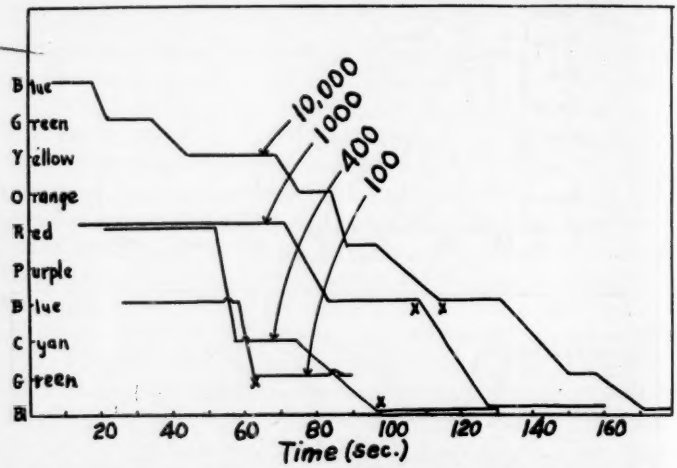


FIG. 5. EFFECTS OF VARYING THE INTENSITY OF THE PRIMARY STIMULUS UPON THE FLIGHT OF COLORS

The point at which the after-image turns negative is indicated by X. The curves are for four intensities: 10,000, 1,000, 400, and 100 c. per sq. ft.

*Theory.* The only theory which attempts to explain the phenomenon of flight of colors is that offered by Helmholtz. Others have attempted modifications of this theory which have not changed it fundamentally. Helmholtz based his theory on his three-component theory of color vision which he has stated as follows: "The eye is provided with three distinct sets of nervous fibers. Stimulation of the first excites the sensation of red, stimulation of the second, the sensation of green, and the stimulation of the third the sensation of violet."[13] In connection with the flight of colors, Helmholtz assumed that a white object stimulates all three sets of fibers, and that after the withdrawal of the stimulus the gradual decay of the residual stimulation and the recovery from fatigue proceed at different rates in the different systems, lead first to a dominance in one and then the other, and thus give a

[13] Helmholtz, *op. cit.*, 246.

cyclic change in color. Although this theory seems to account for the major facts, it is entirely speculative, for the three-component theory itself is not supported by direct anatomical and physiological evidence and the concepts of differential rates of fatigue and residual stimulation are merely modifications attached to the theory to make it account for the flight of colors.

Numerous investigators have reported the phenomenon of involution, but only Barry and Bousfield have attempted an explanation, one which is

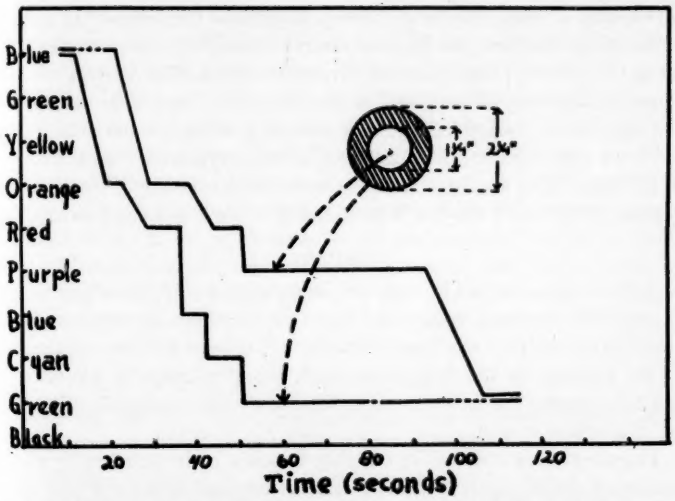


FIG. 6. FLIGHT OF COLORS IN DIFFERENT PARTS OF AN AFTER-IMAGE OF A PRIMARY STIMULUS WHICH CONSISTS OF PARTS HAVING DIFFERENT BRIGHTNESS

The stimulus was observed, at a distance of 16 in. for 20 sec. The brightness of the inner disk was 400 c. per sq. ft., that of the annulus 200 c. per sq. ft.

given in terms of Hecht's theory of photoreceptor processes but which involves in addition the concept of diffusion. They posit three photo-sensitive substances in the retina which react to red, green, and blue light. The light decomposes each of these into three substances P, A, and B of which P is the one which excites the nerve endings. A and B tend to diffuse away from the stimulated area, so that, after the stimulus had ceased, P would be unable to recombine to form S, and P continues to excite. The excitation ceases when A and B diffuse back and unite with P to form S. The involution of color is produced by a differential rate of diffusion for the three systems.

The involution phenomenon seems capable of being explained in a

simpler way, that is to say, without the aid of the concept of diffusion of substances from one part of the retina to another. When two parts of the retina are stimulated with different intensities, the flight of colors in the less intensely stimulated area lags behind the more intensely stimulated one. This fact may be conveniently demonstrated by exposing the eye to a bright disk surrounded by a less bright annulus. The results of such an observation are presented in Fig. 6. The involution phenomenon is quite pronounced when a blurred stimulus is used, such as the one described by McDougall, in which the center is very bright and the intensity is graded to the periphery. Here the flight of colors progressively lags from the center to the periphery and gives the involution appearance. An area of uniform intensity may be supposed to give rise to an involution due to the fact that the eye, during the period of fixation, is subject to movements, and there is a zone at the border which receives less stimulation than the center of the image. The involution with uniform area occurs only if the intensity is great. Why it fails to occur if the intensity is weak is difficult to explain.

## SUMMARY

After-images observed in complete darkness have been considered as due to excitation persisting in the retina, and these have been differentiated from after-images observed in a bright field which have been attributed to fatigue. If the intensity of the field upon which the after-image is projected is properly chosen, the fatigue and persistence effects cancel each other and no after-image is seen.

The after-image observed in complete darkness is characterized by a succession of colors which in general remains constant both from one *O* to another, and, with the same *O*, from one observation to another, provided the stimulus-conditions are kept constant. When either intensity or duration is increased, each stage in the progression is delayed and more colors are added at the beginning of the sequence. Moreover, the after-image turns negative at a later stage in the progression.

The flight of colors can be accounted for in Helmholtz's three-component theory by assuming a differential rate of decay of residual stimulation and recovery from fatigue.

The involution can be attributed to eye-movements which shift the image of the primary stimulus from one part of the retina to another, thus stimulating the central zone more than the periphery. A stimulus which is graded from the center to the periphery gives a marked involutional effect.

# A GROUP EXPERIMENT UPON THE AFFECTIVE REACTION TO ODORS

By PAUL THOMAS YOUNG, University of Illinois

In a previous experiment 100 college students (50 men and 50 women) were brought one at a time into the laboratory and there given a standardized affective reaction-test.[1] Odors were presented; the *O*s reported immediately whether they liked or disliked the smells. The test was given also to 100 school children taken one at a time into a special testing room.

The availability of a group of government-employed (F.E.R.A.) students made it possible to repeat the affective reaction-test for 9 days upon a group of 48 men and 8 women. The aims of the present experiment were: (1) to see whether the affective reaction-test could be administered adequately under group conditions and to compare results of group and individual tests; (2) to obtain more data for our norms of affective reaction; and (3) to study further the variability of affective reactions both within a group and for the same *O* on successive days. The study is part of a more comprehensive investigation of moods; the mood study will be reported elsewhere.

## METHOD AND PROCEDURE

*Odors.* The odors used in this experiment were 14 of the original standard series of 16. These odors will be designated by number as follows.

(1) Camphor, 13 grm.
(2) Methyl salicylate, 26 grm.
(3) Vanillin, 11 grm.
(4) p-Dichlorobenzene, 16 grm.
(5) Menthol, 13 grm.
(6) Phenol, 18 grm.
(7) Acetophenone, 21 grm.
(9) Geraniol, 27 grm.
(10) Ethyl cinnamate, 28 grm.
(11) n-Caproic acid, 17 grm.
(12) Quinoline, 18 grm.
(13) Heptyl aldehyde, 16 grm.
(14) o-Bromotoluene, 29 grm.
(16) Distilled water, control.

*Instructions and procedure.* The *O*s were spaced in the first, third, fifth, seventh, and ninth rows of chairs, and the even numbered rows of chairs were pushed forward so that assistants, bearing the bottles of odorous substance, could move freely about the room. The *O*s were told at once that the experiment dealt with the liking and disliking of odors. The following special instructions were read and repeated on every day of the experiment.

Bottles of odorous substances will be passed around the room. Some will be pleasing, some displeasing. None are injurious to the slightest degree. Because the

* Accepted for publication June 11, 1936.

[1] E. H. Kniep, W. L. Morgan, and P. T. Young, Studies in affective psychology: XI. Individual differences in affective reaction to odors; XII. The relation between age and affective reaction to odors, this JOURNAL, 1931, 43, 406-421.

sense of smell becomes adapted to odors it will be necessary to pause between successive presentations of the odors. Wait quietly between presentations.

When you see an assistant nearing you with the bottle of odorous substance *close the eyes*. When he says "ready" inhale twice, turning the head slightly aside for exhaling (to avoid breathing into the bottle).

Immediately after this write down whether you *like* or *dislike* the odor, using the sign + to indicate liking and the sign — for disliking. If you are completely indifferent, put down a + or a — anyway; then show your uncertainty by adding a question mark (?).

Two assistants passed the odors around the room. They were instructed as follows.

In presenting an odor to *O* say "ready," then pause one and a half to two seconds before placing the bottle under his nose (count mentally "one," "two," "three," after the ready signal). Make sure that *O*'s eyes are closed during the presentation and that the bottle comes directly under his nose but without contact. Watch *O*'s head movements and respiration; wait until he has taken two inhalations. Then pass quickly to the next *O*.

When every one in the room has smelled an odor bring the bottle back to the desk. Proceed at once to the wash room; wash your hands with soap and water, then return for the next odor.

At the start all went as planned but with repetition of the experiment the procedure became slightly modified in two respects. First, the approach of the assistant proved to be ample warning; the superfluous "ready" signal was dropped. Secondly, with unpleasant odors and to some extent with pleasant ones a single whiff proved to be enough; some of the *O*s did not always take the second inhalation. I believe that neither of these modifications of procedure, which came on so gradually that they were scarcely detected, significantly affected the results. The *O*s all whiffed the odors under satisfactory conditions of presentation and recorded their immediate affective reactions. They coöperated in making the experiment a serious and successful one.

Every *O* was given a blank sheet of paper. On this he wrote his assumed name (which was kept constant throughout the experiment) and the date. On the left he wrote numbers one to fourteen and beside each number he entered his judgment as +, +?, —?, or —.

*Order of presenting the odors.* It has been shown by Beebe-Center and others that the general affective value of an odor varies with its position in a series of pleasing and displeasing odors.[2] Consequently it would be highly desirable to hold to our standard order of presentation throughout this experiment. But there is a serious difficulty with such a plan. If the same order of presentation were employed day after day, the subjects would learn the sequence; they would anticipate the odors and their previous affective reactions to them.

The sequences which we employed represent a compromise between a desire to retain the standard presentation order day after day, and a desire to vary it by chance so that recognition and anticipation of sequences would be wholly avoided.

As a matter of record the orders of presentation are reproduced below. The date of the experiment is indicated at the top (all dates were in January 1935) and the sequence employed on that date is listed below. Read down each column.

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[2] J. G. Beebe-Center, The law of affective equilibrium, this JOURNAL, 41, 1929, 54-69.

There was always at least 1 min. between successive presentations. This was controlled by delaying one of the assistants when necessary before permitting him to start on his trip around the room. We assumed that, with a relatively long delay between presentations and with a different serial order on every day, affective contrast and habituation effects would be largely balanced out. The results which we obtained well justify the assumption.

The *Os* were instructed to record immediately after the presentation whether they *liked* or *disliked* the odor. They were further instructed to indicate liking or dis-

| 21st | 22nd | 23rd | 24th | 25th | 28th | 29th | 30th | 31st |
|---|---|---|---|---|---|---|---|---|
| 9 | 3 | 14 | 7 | 1 | 9 | 3 | 3 | 9 |
| 2 | 1 | 10 | 12 | 3 | 12 | 1 | 1 | 12 |
| 7 | 13 | 6 | 9 | 9 | 7 | 13 | 13 | 7 |
| 4 | 5 | 16 | 3 | 12 | 14 | 5 | 5 | 14 |
| 10 | 12 | 4 | 1 | 7 | 10 | 2 | 2 | 10 |
| 6 | 14 | 2 | 13 | 14 | 6 | 4 | 4 | 6 |
| 16 | 11 | 11 | 5 | 10 | 16 | 11 | 11 | 16 |
| 11 | 16 | 5 | 11 | 6 | 11 | 16 | 16 | 11 |
| 14 | 6 | 13 | 2 | 16 | 4 | 6 | 6 | 4 |
| 12 | 10 | 1 | 4 | 11 | 2 | 10 | 10 | 2 |
| 5 | 4 | 3 | 16 | 4 | 5 | 14 | 14 | 5 |
| 13 | 7 | 9 | 6 | 2 | 13 | 7 | 7 | 13 |
| 1 | 2 | 12 | 10 | 5 | 1 | 12 | 12 | 1 |
| 3 | 9 | 7 | 14 | 13 | 3 | 9 | 9 | 3 |

liking even if in doubt, but to record uncertainty by a question mark. We had hoped that such an instruction would force all reports into the negative or positive category, but a few *Os* recorded doubt without any qualification. The symbols used by the *Os* and their meaning are: + = I like it; +? = I like it, uncertain; ? = Wholly uncertain, indifferent; −? = I dislike it, uncertain; and − = I dislike it.

For every odor in the series a percentage of pleasantness was computed by the formula: %P = 100 (P + I/2) / (P + I + U). In computing the percentage of pleasantness all of the uncertain categories of judgment were treated as indifferent (I).

*Results. (1) Frequency of affective judgments.* In Table I the odors are arranged in the column at the left from the most to the least pleasing on the basis of the percentages of pleasantness. Every percentage is based upon 312 judgments.[3] The frequency for each category of affective judgment is indicated in the table.

An inspection of the total frequencies for each category of judgment reveals that there are 2311 U reports as against 1771 P, an excess of 540 U reports. We had hoped to obtain an even balance, but in this kind of work one frequently finds a preponderance of U reports.

Despite the surplus of U judgments, the balance swings in the other direction as soon as we limit consideration to the categories of doubt.

[3] If the 56 *Os* had completed the test on all 9 days, there would be 504 reports for each odor. At the close of the experiment there was a decided decline in attendance (see Table IV).

Against 77 U-doubtful (—?) reports there are 197 P-doubtful (+?), an excess of 120 in favor of P-doubtful. One would expect to find more U than P *doubtful* reports inasmuch as there are more U than P *certain* reports. The facts are the reverse of this expectation. The statistical tendency to report P when in doubt rather than U is clearly apparent in the judgments upon the bottle of distilled water (No. 16); but if this control is thrown out of the series, there are still twice as many P judgments as U within the doubtful categories. We are forced to conclude that when the affective reaction is weak or the judgment uncertain the *O*s are inclined to respond positively, to express a like rather than a dislike.

TABLE I

FREQUENCY OF THE USE OF EACH CATEGORY OF JUDGMENT

| Odor No. | Category of judgment | | | | |
|---|---|---|---|---|---|
| | + | +? | ? | −? | − |
| 2 | 290 | 4 | 1 | 1 | 16 |
| 3 | 277 | 2 | 0 | 2 | 31 |
| 9 | 210 | 13 | 0 | 5 | 84 |
| 5 | 196 | 16 | 0 | 0 | 100 |
| 16 | 123 | 108 | 4 | 32 | 45 |
| 4 | 187 | 12 | 0 | 2 | 111 |
| 1 | 147 | 7 | 0 | 4 | 154 |
| 6 | 119 | 10 | 0 | 11 | 172 |
| 7 | 79 | 4 | 2 | 10 | 217 |
| 14 | 54 | 7 | 0 | 5 | 246 |
| 10 | 54 | 7 | 0 | 2 | 249 |
| 12 | 15 | 3 | 1 | 2 | 291 |
| 13 | 11 | 2 | 3 | 1 | 295 |
| 11 | 9 | 2 | 1 | 0 | 300 |
| Totals | 1771 | 197 | 12 | 77 | 2311 |

*(2) Comparison with previous study.* The preferential order given in Table I has been correlated with that obtained in the earlier study.[4] The coefficient of correlation computed by the rank-difference method of Spearman has the positive value of 0.93 which is decidedly high. In the former experiment coefficients of correlation were computed among three age-groups: (a) a group of 50 children ages 7-9 yr.; (b) a group of 50 children ages 11-13 yr.; and (c) a group of 100 college students. The three coefficients of correlation had the values of 0.93, 0.92, 0.98. The only possible interpretation is that likes and dislikes for odors depend primarily upon organic factors which are relatively independent of age and not greatly affected by environmental conditions, social or other.

Beebe-Center has shown this by another method and concludes that "in

[4] Kniep, Morgan, and Young, *op. cit.*, 408.

the case of relative affective judgments by different individuals upon a single set of olfactory stimuli agreement is primarily dependent upon like organic constitution whereas disagreement rests in the main upon unlike past experience."[5] This conclusion, of course, in no way contradicts the fact that we *learn* to like and dislike certain odors and to associate them with pleasing and displeasing situations.

Comparing the actual percentages of pleasantness with those of the former group of college students, odor by odor, we discover that the present group shows a decline in the P percentage of ten odors (declines of —4, —21, —11, —21, —28, —12, —1, —8, —6, —3%) and a gain with four others (gains of +4, +3, +1, +10%). The declines are more numerous than the gains, and greater in sum, but whether these changes are merely chance variations or whether they have some true and hidden significance, dependent upon differences in the experimental conditions, we cannot say.

*(3) The pleasantness of smelling distilled water.* One would expect the affective judgments upon distilled water (No. 16, used as a control presentation) to be indifferent, 50% P and 50% U. The fact is that the control presentation is judged P more frequently than U. If we ignore all doubtful judgments, it is 73.2% P. If we group the P-doubtful judgments with P and the U-doubtful with U, the distilled water rates 74.7% P. For the sake of comparison we computed a percentage of pleasantness on the basis of P-doubtful and U-doubtful judgments alone, ignoring all certain judgments; on this basis the control was 77.1% P.

If we work out a percentage of pleasantness on the basis of the above formula (treating all doubtful reports as indifferent), the control presentation has a value of 62.5% P. This last value is nearly the same as the rating of 61% P given to an *empty bottle* control in the earlier study with the same odors.[6] Any way we look at the figures the control appears to be above the expected value of 50% P. (See the daily variations of No. 16 in Table IV.)

The interpretation of this result is a matter of opinion. In the standard series the control comes immediately after several unpleasant odors; this suggests that the phenomenon might be one of affective contrast.[7] Although we formerly suggested the hypothesis of affective contrast, we are inclined now to abandon it in interpreting the phenomenon in question. For one

[5] Beebe-Center, The variability of affective judgments upon odors, *J. Exper. Psychol.*, 14, 1931, 91-93.
[6] Kniep, Morgan, and Young, *op. cit.*, 413.
[7] *Ibid.*, 412.

thing, we did not hold to the standard series but varied it from day to day; despite such variation the control was never below the 50% point.

I think that the unexpectedly high percentage of pleasantness with the control presentation should be referred to the intellectual processes of judgment rather than to any affective principle. There are several lines of evidence that point to this conclusion.

*(a)* In the first place, the percentage of pleasantness is higher for the doubtful (77.1% P) than for the certain (73.2% P) reports of affective reaction to the control. In other words, the positive responses to the distilled water are more frequent when the affective experience is weak or uncertain than when there is no doubt. It is clear that affective uncertainty favors guessing.

*(b)* In the second place, it has been said that when in doubt we are more likely to react positively than negatively. It is easier, more "natural," more in the line of least resistance, and generally more agreeable to say *yes* than *no*.[8]

*(c)* In the third place, the percentage of pleasantness for the control presentation is higher with children than adults. Miss Morgan found that with children of 7-9 years the empty bottle was 82% P; with children of 11-13 years the control rated 67% P.[9] With college students, as noted above, the control (empty bottle or distilled water) rated 61% or 62% P. Young children are known to be suggestible; a number of them said they liked all of the odors (100% P); a few, on the contrary, reported that they disliked all of them (0% P). The results clearly indicate a factor of suggestibility which complicates the reports upon affective reactions.

All things considered I believe it is best to conclude that the judgments summarized in Table I depend primarily upon affective reactions within the subjects, but that in addition to the affective reactions the reports are partly determined by the intellectual processes of judgment and especially by some factor which predisposes the subject to make a positive rather than a negative report.

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[8] I have recently put this principle to an experimental test. A final examination in psychology contained a large number of questions to be answered with *yes* or *no*. I asked the students to indicate before turning in their papers which of the answers were *pure guesses*, and assured all that this information would in no way affect their grades upon the examination. The results were tabulated as follows to reveal which answers were by chance correct, which incorrect, which were guessed *yes*, which *no:*

| | Answered "yes" | Answered "no" | Total answers |
|---|---|---|---|
| Guess was correct | 219 | 261 | 480 |
| Guess was incorrect | 228 | 86 | 314 |
| Total guesses | 447 | 347 | 794 |

A study of these figures reveals two things: (a) that the students have about 60 chances in 100 of getting the answer correct even when they believe that their answer is a pure guess; (b) that in guessing there are about 56 chances in 100 that the answer will be positive rather than negative. We do, then, tend to react positively rather than negatively when in doubt. Unfortunately no record was kept of the number of these questions which should have been answered by *yes* and by *no*.

[9] Kniep, Morgan, and Young, *op. cit.*, 416 f.

*(4) Affective reaction-scores.* For every *O* an affective reaction-score was determined by adding algebraically the number of odors reported as *liked* to the number reported as *disliked*. Inasmuch as there were always 14 presentations in a given test, the highest possible score is +14 (all reports of P) and the lowest —14 (all reports of U). The actual scores did not reach the extremes but ranged between +13 and —10. All doubtful reports were assumed to be ½ P and ½ U.

The daily affective reaction-scores for all the *O*s present on four or more of the nine experimental days were averaged. Table II presents these average affective reaction-scores ranked in order from the highest (+8.0)

TABLE II

AFFECTIVE REACTION-SCORES

| No. days | Av. score | A.D. | No. days | Av. score | A.D. | No. days | Av. score | A.D. |
|---|---|---|---|---|---|---|---|---|
| 6 | +8.0 | 1.3 | 7 | +0.1 | 1.3 | 5 | −4.2 | 1.0 |
| 4 | +7.2 | 3.1 | 5 | −0.4 | 0.6 | 6 | −4.3 | 1.0 |
| 4 | +4.5 | 1.7 | 6 | −0.5 | 2.2 | 4 | −4.5 | 1.0 |
| 5 | +4.2 | 1.4 | 4 | −0.5 | 1.0 | 4 | −4.5 | 1.7 |
| 6 | +3.0 | 4.0 | 8 | −1.2 | 1.0 | 7 | −5.0 | 1.4 |
| 5 | +3.0 | 2.4 | 9 | −1.3 | 1.6 | 6 | −5.0 | 2.6 |
| 7 | +2.8 | 1.5 | 5 | −1.4 | 2.5 | 6 | −5.5 | 1.5 |
| 6 | +2.8 | 1.6 | 6 | −1.6 | 0.8 | 5 | −5.6 | 1.5 |
| 5 | +2.4 | 1.5 | 9 | −1.8 | 1.8 | 9 | −5.7 | 1.9 |
| 5 | +1.6 | 2.7 | 7 | −1.8 | 1.9 | 7 | −6.3 | 2.0 |
| 6 | +1.5 | 3.1 | 8 | −2.2 | 1.3 | 8 | −6.7 | 0.8 |
| 9 | +1.1 | 1.9 | 7 | −2.4 | 0.5 | 4 | −7.0 | 2.0 |
| 9 | +1.0 | 0.9 | 9 | −2.8 | 2.0 | 9 | −7.1 | 0.6 |
| 8 | +0.6 | 2.9 | 9 | −3.3 | 0.9 | 9 | −9.4 | 0.5 |
| 9 | +0.5 | 1.8 | 7 | −4.0 | 2.5 | 5 | −9.4 | 0.9 |

to the lowest (—9.4). The table also indicates the number of days upon which the test was given for every *O*, and the average deviation for each average score.

Table II reveals that there are more *minus* than *plus* affective reaction-scores for the group as a whole. The original standard series was selected to give approximately equal frequencies of P and U reports. The present group of *O*s is actually more negative than the former adult group. Whether the difference between the present and the former results can be referred to some change of procedure, to a variation within the stimuli, to the sex-distribution of the *O*s comprising the group, or to some other obscure condition, is purely a matter of conjecture.

Of the scores shown in Table II the average is —1.5 and the median is —1.6. The preponderance of negative affective reaction-scores is shown again in the following manipulation of results.

*(5) Day to day distribution of affective reaction-scores.* Table III presents the distribution of individual affective reaction-scores day by day. The table should be read as follows: On January 21, the first day of the experiment, there were two individuals in the group with an affective reaction score of —10, one with a score of —9, three with a score of —8, etc. On January 22, the second day, there was one individual with a score of —10, one with a score of —9, etc.

If we compute the averages and medians for the group day by day, we arrive at the following values. It will be seen at once that these values are

TABLE III

DISTRIBUTION OF INDIVIDUAL AFFECTIVE REACTION-SCORES

| Date (Jan.) | Affective scores −10 | −9 | −8 | −7 | −6 | −5 | −4 | −3 | −2 | −1 | 0 | +1 | +2 | +3 | +4 | +5 | +6 | +7 | +8 | +9 | +10 | +11 | +12 | +13 |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| 21 | 2 | 1 | 3 | 2 | 5 | 2 | 6 | 6 | 5 | 2 | 4 | 3 | 0 | 1 | 2 | 0 | 0 | 0 | 1 | 0 | 0 | 1 | 0 | 0 |
| 22 | 1 | 1 | 6 | 1 | 2 | 4 | 2 | 3 | 9 | 3 | 2 | 2 | 4 | 1 | 2 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 1 |
| 23 | 1 | 1 | 1 | 4 | 2 | 3 | 3 | 4 | 5 | 4 | 3 | 2 | 5 | 1 | 0 | 1 | 1 | 0 | 1 | 0 | 1 | 0 | 0 | 1 |
| 24 | 2 | 0 | 1 | 3 | 3 | 1 | 2 | 6 | 4 | 2 | 5 | 3 | 2 | 1 | 0 | 2 | 1 | 2 | 1 | 0 | 0 | 1 | 0 | 0 |
| 25 | 1 | 2 | 0 | 3 | 0 | 1 | 2 | 2 | 5 | 3 | 3 | 3 | 3 | 2 | 3 | 2 | 1 | 0 | 1 | 1 | 1 | 0 | 0 | 0 |
| 28 | 2 | 0 | 0 | 4 | 1 | 2 | 1 | 5 | 1 | 2 | 2 | 4 | 2 | 3 | 1 | 2 | 0 | 1 | 1 | 0 | 0 | 0 | 0 | 0 |
| 29 | 1 | 1 | 0 | 2 | 1 | 2 | 3 | 5 | 3 | 3 | 1 | 1 | 1 | 1 | 1 | 1 | 1 | 1 | 1 | 0 | 0 | 0 | 0 | 0 |
| 30 | 0 | 1 | 1 | 1 | 2 | 2 | 4 | 0 | 1 | 2 | 1 | 1 | 0 | 0 | 1 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 |
| 31 | 0 | 1 | 1 | 2 | 1 | 0 | 0 | 0 | 1 | 1 | 2 | 5 | 2 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 |
| Total | 10 | 8 | 13 | 22 | 17 | 17 | 23 | 31 | 34 | 22 | 23 | 24 | 19 | 10 | 10 | 8 | 4 | 4 | 6 | 1 | 2 | 2 | 0 | 2 |

| Date | Av. affective score | Median affective score |
|---|---|---|
| Jan. 21 (M) | −2.8 | −3.6 |
| Jan. 22 (T) | −2.5 | −2.8 |
| Jan. 23 (W) | −1.4 | −2.4 |
| Jan. 24 (T) | −1.2 | −1.7 |
| Jan. 25 (F) | −0.3 | −0.7 |
| Jan. 28 (M) | −1.1 | −1.5 |
| Jan. 29 (T) | −1.6 | −2.5 |
| Jan. 30 (W) | −3.6 | −4.5 |
| Jan. 31 (T) | −1.9 | −0.5 |

all negative. The average for the total distribution is —1.73 (S.D. 4.6), and the median for the total distribution is —2.6 (S.D. 4.6).

The group averages and medians show that during the first five days of the experiment, Monday to Friday, there was a progressive change in affective reaction from unpleasantness toward indifference. Over the week-end the trend ceased and there were irregular variations during the last four days. The interpretation of these changes is uncertain.

*(6) Stability of affective value of the odors.* If we treat the group of subjects collectively as a unit and compute a percentage of pleasantness for each odor in the series, we arrive at the values recorded in Table IV. The table should be read as follows: On January 21 odor number 2 had a percentage of pleasantness of 92.4 for the total group, on January 22 the

same odor had a percentage of pleasantness of 95.4, etc. The percentages were computed by the above formula; all doubtful reports were treated alike as indifferent.

The odors are ranked in an order which depends upon the percentage of pleasantness for the total results.

TABLE IV

DAY TO DAY VARIATION IN THE PERCENTAGE OF PLEASANTNESS

| Odor number | Date and number of *Os* | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|
| | Jan. 21 | 22 | 23 | 24 | 25 | 28 | 29 | 30 | 31 |
| | No. 46 | 44 | 44 | 41 | 39 | 34 | 30 | 17 | 16 |
| 2 | 92.4 | 95.4 | 96.6 | 95.1 | 94.9 | 95.5 | 95.0 | 88.2 | 84.3 |
| 3 | 91.3 | 79.3 | 87.5 | 94.0 | 84.6 | 91.2 | 96.7 | 91.1 | 93.7 |
| 9 | 75.0 | 64.8 | 67.0 | 71.4 | 75.6 | 75.0 | 70.0 | 50.0 | 75.0 |
| 5 | 61.9 | 53.4 | 75.0 | 69.0 | 71.8 | 61.8 | 66.7 | 61.8 | 65.6 |
| 16 | 50.0 | 71.6 | 65.9 | 64.3 | 64.1 | 55.9 | 66.6 | 55.8 | 68.7 |
| 4 | 48.9 | 67.0 | 67.0 | 64.3 | 78.2 | 69.1 | 48.3 | 47.0 | 75.0 |
| 1 | 42.4 | 32.9 | 53.5 | 51.2 | 60.2 | 60.3 | 51.6 | 38.2 | 46.8 |
| 6 | 27.2 | 40.9 | 40.9 | 52.4 | 48.7 | 47.0 | 38.3 | 23.5 | 53.1 |
| 7 | 26.1 | 21.6 | 27.3 | 30.9 | 41.0 | 26.5 | 28.3 | 35.3 | 12.5 |
| 14 | 13.0 | 23.8 | 26.1 | 15.4 | 25.6 | 26.5 | 16.6 | 00.0 | 9.4 |
| 10 | 14.1 | 13.6 | 18.2 | 17.8 | 25.6 | 17.6 | 28.3 | 29.4 | 6.2 |
| 12 | 6.5 | 6.8 | 2.3 | 5.9 | 10.2 | 2.9 | 8.3 | 00.0 | 3.1 |
| 13 | 5.4 | 2.3 | 5.7 | 3.6 | 7.7 | 5.9 | 1.6 | 00.0 | 3.1 |
| 11 | 4.4 | 3.4 | 2.3 | 1.2 | 5.1 | 2.9 | 6.7 | 00.0 | 3.1 |

The odors which are highest in affective value and those which are lowest are remarkably constant from day to day, whereas those in the middle range show greater variability. This relative stability at the extremes and variability in the middle range cannot, however, be interpreted psychologically. It is a statistical artifact. It is clear that in order to obtain a stable affective value of 100% P or 0% P the average deviation must necessarily be zero, and that averages near these extremes could not exist at all if the variability were large. In a previous experiment[10] by the scale of values method I encountered a similar difficulty.[11]

## SUMMARY

It has been shown that a test of affective reaction to odors can be administered satisfactorily to a group of subjects with results strikingly similar to those obtained with individual presentation. The rank-order coefficient of correlation for individual and group presentation is positive and

[10] P. T. Young, Constancy of affective judgment to odors, *J. Exper. Psychol.*, 6, 1923, 182-191.

[11] In this connection see Young, Studies in affective psychology, this JOURNAL, 40, 1928, 399 (footnote 22).

high ($r = 0.93$). This result confirms a previous conclusion that likes and dislikes for odors are determined by relatively stable bodily mechanisms.

The range and variability of individual and group affective reaction scores have been examined. The group distributions reveal day to day variations in the average and median affective reaction scores, progressive and regressive changes which as yet are unexplained.

The present group of *Os* is more negative to odors than the previous group tested individually with the same stimuli. This difference appears in distributions of the individual affective reaction scores.

A bottle of distilled water presented in the context of odors was judged distinctly pleasant. This result, we believe, should be explained by reference to the processes of judgment and evaluation rather than by reference to affective reactions.

The odors which are most extremely P or U remain consistently P or U throughout the experiment, whereas affectively intermediate odors show greater variability about an average. This result is a statistical artifact which does not imply that an intense P or U affective reaction is psychologically less variable from day to day than one near indifference.

# THE RÔLE OF INSIGHT IN THE LEARNING OF LOGICAL RELATIONS

By MARY SCHOOLEY, Pennsylvania State College, and
GEORGE W. HARTMANN, Columbia University

In 1926 Dallenbach reported an effort to teach three children the meaning of opposition.[1] He met with immediate success in the case of a 6-yr.-old and complete failure with a 3-yr.-old. Another child 4½ yr. of age did not learn the relationship at first but did later when insight into the task appeared.

In a sequel to this study, Dallenbach and Kreezer performed under standardized conditions, a similar experiment of broader scope.[2] They found that children of the social status of the selected group learn the meaning of opposition without any particular instruction concerning it by the end of their eighth or at the beginning of their ninth year. Children of a 5·3–5·9-yr. age-group have, however, a probability of 0.5 of learning the relation with a small amount of instruction. Those who do master the problem seem to do so not gradually (as required by trial-and-error theories of learning) but "insightfully," *i.e.* by a process involving a "sudden" solution of the difficulty.

A legitimate extension of this type of research appears to be involved in the question: Does a similar situation obtain in the case of other standard logical relations, such as part-whole, action-agent, attribute-substance, genus-species, and the converse of each of these? There are empirical reasons for believing that most of these forms of thought are probably more difficult to comprehend than that of simple opposition. At any rate they seem to be acquired a little later in the course of the ordinary child's career.

The present study is mainly concerned with the manner and time of acquisition of these relations. Two hundred *S*s were examined and an answer to the following questions was sought: (1) How do children who do not know the meanings of standard logical relations learn them—do they learn them gradually, in accordance with the trial-and-error theories of learning,

* Accepted for publication March 5, 1935. This study was performed while the authors were associated in the Department of Psychology of the Pennsylvania State College.

[1] K. M. Dallenbach, A note on the immediacy of understanding a relation, *Psychol. Forsch.*, 7, 1926, 268-269.

[2] George Kreezer and K. M. Dallenbach, Learning the relation of opposition, this JOURNAL, 41, 1929, 432-441.

or suddenly with the appearance of insight?[3] (2) At what age are children in general capable of learning the relations?

*Subjects.* Sex and chronological age were the only bases for the selection of the subjects. For a large number of these, however, Stanford-Binet IQ were known and these, along with the social status of the parents, were probably for the most part slightly above the average. Twenty boys and 20 girls in the community of State College, Pa., were chosen from each of the following age-groups: 5·0–5·6, 5·7–5·12, 6·0–6·6, 6·7–6·12, 7·0–7·6; making 200 *Ss* in all.

*Method and procedure.* Except in a few instances which will be noted, the procedure in this experiment paralleled that used by Dallenbach and Kreezer in order to insure comparable results. The method followed was that of the individual clinical interview, and the first prerequisite in each case was the establishment of rapport with the child.

After preliminary experimentation and following a suggestion made by Dallenbach and Kreezer concerning the amount of training necessary for learning the relation of opposition, it was decided that only 10 words were required in the list for each logical relation instead of 25 as were used in their experiment. This shortened list prevented the test from becoming too time-consuming and fatiguing for the younger children. Since the purpose of the experiment was to find out only whether the child understood or could be made to understand the meaning of various standard logical relations, and not whether he knew the technical names for them (which would have made it a purely vocabulary test), the names for such relations were omitted in all of the questions where simpler terminology seemed more suited to the child's vocabulary.

The technique followed by the examiner in each division of the experiment may be illustrated by the whole-part section. Before beginning the testing proper, the attention and interest of the child were secured. Then the examiner began:

(1) "Would you like to play a game of "whole-part" this morning?" After the child's assent, the examiner continued: "Do you know what whole-part means?"

A

(2) To a *negative* answer, the examiner said, "Yes, you do. Tell me what is part of an *apple.*"
To an *affirmative* answer, the examiner said, "All right, tell me what is part of an *apple.*"

(3) To an incorrect answer, the examiner said, "No, you are wrong, that is not part of an apple. Now tell me what is part of a *clock.*"
To a correct answer, the examiner said, "That's right, now tell me what is part of a *clock.*"

B

(4) To an incorrect answer, the examiner said, "No, you are wrong. Now listen, part of an apple is a seed; part of a clock is the hands. Do you understand?"
To a correct answer, the examiner said, "That's right, part of an apple is a seed; part of a clock is the hands. Do you understand?"

C

(5) To a negative answer in question 4, the examiner said, "Well, I'll see if you do. Tell me what is part of a *book* (*hat, pencil, hand, dog, chair, train, house, automobile, shoe*)."

[3] For a discussion of what is indicated here by 'insight' see G. W. Hartmann, The concept and criteria of insight, *Psychol. Rev.*, 38, 1931, 242-252.

To an affirmative answer the examiner said, "All right, I'll see if you do. Tell me what is part of (the words listed above)."

D

(6) As the words were given to the *S* and he returned his answers, *E* acknowledged them with a "right" or "wrong." If *S* did not show by his answers to the first 6 words of the test list that he understood the relationship, the examiner again told him the correct answer to *apple* and *clock* and then repeated the first 6, saying "right" when a correct answer was returned and supplying the correct answer when an error was made. The remaining 4 words were given without any correction or comment on the *S*'s replies.

The standard designated for success in learning the relations in each case was one correct reply to any one of the first six words on the test list and all correct responses to the remaining words.

Standard logical relations other than the whole-part one used in the experiment were: part-whole, action-agent, agent-action, attribute-substance, substance-attribute, genus-species, and species-genus.

## RESULTS

*Manner in which the relationships were learned.* Table I summarizes the data obtained from all of the interviews. Results for girls and for boys are tabulated separately. The captions across the top of the table refer to the eight standard logical relations used in the experiment. The Parts A to E indicate the number and nature of answers received during the testing procedure as outlined in the six steps given immediately above. Answers to the first question are omitted from tabulation because they were almost invariably, "No."

An average of the number of correct responses to the first two test words, numbered (2) and (3) under the testing procedure, for each relationship was computed and is listed under Part A. Then opposite Part B is recorded the number of children who replied that they did understand each of the relations after the two illustrative words had been given to them.

In Part C is listed the number of *S*s who succeeded in getting correct one of the first six words in the test list and all of the four remaining. This part illustrates two important points: (1) in every case a larger number of the *S*s said they understood the relationship than were able to prove that they did by correct responses to the stimulus-words; and (2) a very large proportion of the children appear to have learned the relationships after the use of only two illustrative examples.

When the test list was repeated as indicated in Part (6) of the testing procedure, a few more children gave evidence that they had learned the relationships—not by any drill-like trial-and-error process but apparently by means of insight, since the correct response was acquired suddenly and continued throughout the remainder of the list. The development of an

insight into the problem was often accompanied by a triumphant, "Oh, I catch on now!"

Parts C and D are totaled and entered under Part E to show the total number of those tested who were able to learn the relations from the limited amount of training given.

In every case except that of the genus-species relationship, totals in Part E are substantially higher than Part A, indicating that a relatively large proportion of the children learned, apparently by insight, a number of re-

TABLE I

RESULTS OF THE SEQUENTIAL STEPS IN LEARNING CERTAIN STANDARD LOGICAL RELATIONS

| Steps | Sex | Part-whole | Whole-part | Action-agent | Agent-action | Attrib.-sub. | Sub.-attrib. | Genus-species | Species-genus |
|---|---|---|---|---|---|---|---|---|---|
| A | Boys | 18 | 18 | 55 | 55 | 14 | 20 | 56 | 10 |
| | Girls | 20 | 18 | 43 | 53 | 11 | 16 | 63 | 7 |
| B | Boys | 81 | 83 | 90 | 89 | 77 | 70 | 76 | 69 |
| | Girls | 83 | 80 | 91 | 94 | 87 | 72 | 81 | 77 |
| C | Boys | 45 | 43 | 80 | 80 | 55 | 60 | 50 | 32 |
| | Girls | 43 | 45 | 80 | 82 | 54 | 55 | 56 | 31 |
| D | Boys | 9 | 11 | 5 | 5 | 10 | 5 | 8 | 7 |
| | Girls | 9 | 8 | 4 | 1 | 10 | 5 | 3 | 6 |
| E | Boys | 54 | 54 | 85 | 85 | 65 | 65 | 58 | 39 |
| | Girls | 52 | 53 | 84 | 83 | 64 | 60 | 59 | 37 |

lationships which they did not know at the beginning of the experiment. The negative results obtained for the genus-species relationship may probably be accounted for by the fact that a large number of the *Ss* responded "red" to the word "color" without understanding the relationship implied; they answered as if the two terms were, in their minds, synonymous as a result of frequent mechanical association.

*Ages at which the relationships were learned.* Fig. 1 was prepared by dividing the totals listed under Part E in Table I into the number of *Ss* successful in each of the age-groups tested. These numbers were then converted into percentage values of the total group at each age-level. Limens indicating the ages at which the children were capable of learning each of the standard relations were computed separately for boys and girls.[4] Since these thresholds are expressed as the midpoints of a 6-mo. range, the age-groups at which the average child has a 0.5 probability of learning these relationships extend 3 mo. on either side of the calculated limen.

[4] W. Brown and G. H. Thomson, *The Essentials of Mental Measurement*, 1921, 57-61.

The different heights of the columns in Fig. 1 show the relative difficulty range for the various relations studied. Children of the age-group 5·1–5·7 are able to learn the easiest of the relations while they must belong to a 6·4–6·10-yr. age-group to learn the most difficult of the relations with the amount of training given our group. The relations themselves vary in the following order from easiest to hardest as measured by the increase in the age limens: action-agent (5·4), agent-action (5·4), attribute-substance (5·10), substance-attribute (5·11), genus-species (5·11), whole-part

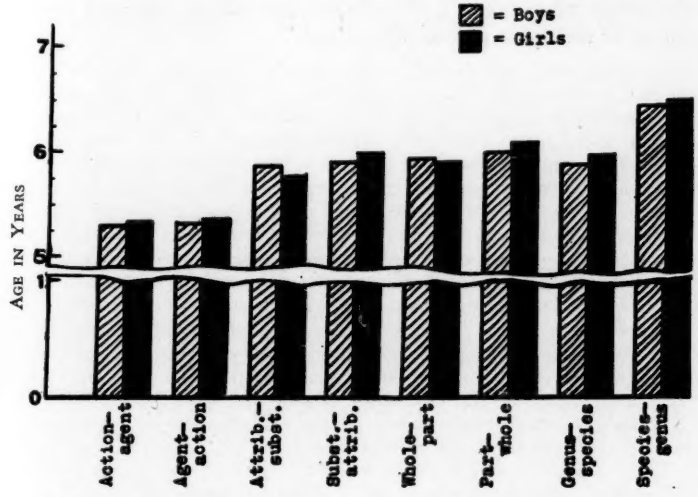


FIG. 1. COMPARISON OF THE LIMENS FOR THE DIFFERENT RELATIONS

(5·11), part-whole (6·1), and species-genus (6·7). Although girls tend to find the relations more difficult than do the boys, the differences between them are too slight to have any statistical significance.

## SUMMARY

The following answers may be given to the problems which this study sought to solve.

*(1)* Learning standard logical relationships is not a slow, gradual process involving practice-effect or trial-and-error responses but occurs suddenly and gives evidence of permanent mastery. When the desired concept is once formed, the child responds 100% correctly to the remaining words

illustrative of the learned relationship, providing they are within the range of his vocabulary.

*(2)* The probable order of difficulty of learning the relations is from easiest to hardest as follows: Action-agent, agent-action; attribute-substance, substance-attribute; genus-species; whole-part, part-whole; and species-genus.

*(3)* With the amount of training here given, children similar to those used have a 0.5 probability of learning the standard logical relations when they are members of age-groups varying from 5·1–5·7 to 6·4–6·10 yr. The narrow range here of 1 yr. and 3 mo. indicates the close interrelationship of all the logical relationships studied.

# THE DOMINANT THUMB IN RELATION TO STUTTERING, EYEDNESS, AND HANDEDNESS

By WENDELL JOHNSON, State University of Iowa

In 1908, Lutz published a study on the manner of clasping the hands.[1] He observed about 600 individuals, probably of Scotch descent for the most part, and concluded that the mode of clasping the hands is inherited. The observation which was made was that of determining whether the right or the left thumb (actually thumb and fingers) was uppermost when the hands were clasped with fingers interlacing. Lutz found that 61% of males and 58% of females placed the right thumb uppermost.

In 1926 and again in 1927, Downey reported observations concerning this matter.[2] Since her 1927 report is the more complete, I shall refer only to it. Downey observed the "thumbedness" of 1205 individuals, 732 men and 473 women. She classified these *Ss* on a rather elaborate basis with respect to handedness, employing a scheme originated by Rife[3] and refined by herself. She then attempted to ascertain whether a relation might exist between degrees of right and left handedness and manner of clasping the hands (it is convenient to refer to this as "thumbedness," and I shall henceforth use the term). We may disregard the subdivisions of her classification in summarizing her findings. The percentage of each group of her *Ss* who were right thumbed was: right handed men, 51.3%; left handed men, 37.1%; right handed women, 57.2%; left handed women, 46.4%. By computing the statistical significances of pertinent differences, she arrived at the conclusion that perhaps a dominant (uppermost) left thumb "appears to indicate latent sinistrality," and that there are sex differences with respect to thumbedness.

The possibility of a relation between handedness and stuttering prompted the present study of the thumbedness of stutterers.[4] My *Ss* were both children and adults, of both sexes, all stutterers, who have been examined in the State University of Iowa Speech Clinic. Of the 271 *Ss*, 227 were males and 44 were females.

Thumbedness was determined simply by having *S* clasp his hands with fingers interlacing, observation being made of the uppermost thumb. This is the procedure followed by Lutz and by Downey. Other data concerning every *S* were also obtained. A handedness history was secured from *S* or his parents. Eyedness was determined by means of the hole-in-paper test.[5] *S*, with both eyes open and

* Accepted for publication June 15, 1936. From the State University of Iowa Speech Clinic.

[1] F. E. Lutz, The inheritance of the manner of clasping the hands, *Amer. Natur.*, 42, 1908, 195 f.

[2] J. E. Downey, Further observations on the manner of clasping the hands, *Amer. Natur.*, 50, 1926, 387-391; Types of dextrality and their implications, this JOURNAL, 38, 1927, 317-367.

[3] J. M. Rife, Types of dextrality, *Psychol. Rev.*, 29, 1922, 474-480.

[4] Wendell Johnson and Lucille Duke, Changes in handedness associated with onset or disappearance of stuttering: Sixteen cases, *J. Exp. Educ.*, 4, 1935, 112-132. L. E. Travis, *Speech Pathology*, 1931, 95-192. L. E. Travis and Wendell Johnson, Stuttering and the concept of handedness, *Psychol. Rev.*, 41, 1934, 534-562.

[5] Johnson and Duke, *op. cit.*, 114.

standing about 10 ft. from *E*, looked at *E*'s nose from 10 to 20 times through a small hole (about the size of a dime) in the middle of a sheet of typewriting paper, which he held in front of his eyes. The number of times each eye was used for this sighting was recorded. The hand preference dextrality quotient (D.Q.) of every *S* was determined by having *S* indicate on a list of unimanual activities those activities for which he preferred to use the right, the left, and either hand.[6] The D.Q. was then ascertained by means of the formula $(R + \frac{1}{2} E)/N$, in which *R* is the number of activities for which the right hand is preferred, *E* is the number of activities for which either hand is preferred (no preference), and *N* is the total number of activities for which a hand preference, either *R*, *L*, or *E*, was indicated.

Results are given in the following tables. Varying totals are due to the fact that for each tabulation only those *S*s were included for whom definitely accurate data were available.

TABLE I

THUMBEDNESS OF STUTTERERS CLASSIFIED ACCORDING TO SEX AND GROSS USAGE HANDEDNESS

| | N | % right thumbed | % left thumbed | D / P.E. diff. | Chances in 100 diff. is significant |
|---|---|---|---|---|---|
| All stutterers | 271 | 48 | 52 | 1.4 | 83 |
| Males | 227 | 46 | 54 | 2.5 | 95 |
| Females | 44 | 59 | 41 | 2.6 | 96 |
| D/P.E.$_{diff.}$ | | (2.4) | | | |
| Chances in 100 that diff. is significant | | (95) | | | |
| Originally left handed | 51 | 33 | 67 | 5.4 | 99+ |
| Males | 43 | 35 | 65 | 4.3 | 99+ |
| Females | 7 | 29 | 71 | | |
| Originally right handed | 139 | 50 | 50 | 0.0 | 50 |
| Males | 116 | 47 | 53 | 1.4 | 83 |
| Females | 23 | 61 | 39 | | |
| D/P.E.$_{diff.}$ | | (3.2)* | | | |
| Chances in 100 that diff. is significant | | (98.5) | | | |
| Originally ambidextrous (all males) | 8 | 63 | 37 | | |
| Present left handed (8 males) | 10 | 40 | 60 | | |
| Present right handed | 144 | 47 | 53 | 1.5 | 84 |
| Males | 123 | 45 | 55 | 2.5 | 95 |
| Females | 21 | 57 | 43 | | |
| Present ambidextrous (15 males) | 19 | 32 | 68 | | |

* This C. R. is with reference to all originally right handed as against all originally left handed *S*s.

## SUMMARY AND CONCLUSIONS

Statistical treatment of differences between the various groups of *S*s in this study indicates the following conclusions.

*(1)* There are no highly significant differences with respect to thumbedness between our 271 stutterers and the 1205 essentially non-stuttering right and left

[6] Wendell Johnson and Darlene Duke, The dextrality quotients of fifty six-year-olds with regard to hand usage, *J. Educ. Psychol.*, 27, 1936, 26-36.

TABLE II

EYEDNESS OF RIGHT AND LEFT THUMBED STUTTERERS OF BOTH SEXES

| | N | % right eyed* | % left eyed |
|---|---|---|---|
| Right thumbed | 128 | 66 | 34 |
| Left thumbed | 135 | 61 | 39 |
| $D/PE_{diff.}$ | (1.3) | | |
| Chances in 100 that diff. is significant | | (81) | |

* Of the 135 left-thumbed individuals, 75 were 100% right eyed (invariably sighted with the right eye) and 43 were 100% left eyed, while 17 fell between these extremes. The distribution was so definitely bimodal that those Ss scoring over 50% right eyed and those scoring over 50% left eyed were classified as right and left eyed respectively. The same procedure was followed with respect to the right thumbed Ss. Of the 128 of these, 68 were 100% right eyed and 33 were 100% left eyed, while 27 fell between these extremes.

TABLE III

HAND PREFERENCE DEXTRALITY QUOTIENT MEANS, STANDARD DEVIATIONS AND RANGES OF RIGHT AND LEFT THUMBED STUTTERERS OF BOTH SEXES

| | Hand preference Dextrality quotient | | | |
|---|---|---|---|---|
| | N | A.M. | S.D. | Range |
| Right thumbed | 100 | 86.0 | 30.01 | 17.0–100 |
| Left thumbed | 113 | 84.6 | 24.09 | 1.4–100 |

TABLE IV

THUMBEDNESS OF STUTTERERS AS COMPARED WITH DOWNEY'S ESSENTIALLY NON-STUTTERING SS CLASSIFIED ACCORDING TO SEX AND GROSS USAGE HANDEDNESS

| | N | % right thumbed | $\frac{D}{P.E._{diff.}}$ | Chances in 100 diff. is significant |
|---|---|---|---|---|
| Downey's right handed males | 589 | 51 | 1.9 | 90 |
| Stuttering males | 227 | 46 | | |
| Downey's left handed males | 143 | 37 | 2.6 | 96 |
| Downey's right handed females | 374 | 57 | 1.2 | 79 |
| Stuttering females | 44 | 59 | | |
| Downey's left handed females | 99 | 46 | 2.2 | 93 |
| All Downey's right handed Ss | 963 | 54 | 2.9 | 97.5 |
| All stutterers | 271 | 48 | | |
| All Downey's left handed Ss | 242 | 41 | 2.4 | 95 |
| Downey's right handed males | 589 | 51 | 1.2 | 79 |
| Originally right handed stuttering males | 116 | 47 | | |
| Downey's left handed males | 143 | 37 | 0.4 | 51 |
| Originally left handed stuttering males | 43 | 35 | | |
| All Downey's Ss | 1205 | 51 | 1.4 | 83 |
| All stutterers | 271 | 48 | | |

handed *Ss* studied by Downey. The stutterers resemble the right handed non-stutterers more than they resemble the left handed non-stutterers, when the sexes are considered separately. This tendency is reversed, but not to a statistically significant degree, when the two sexes are considered together. There are practically no differences between the originally right handed stuttering males and Downey's right handed males, or between the originally left handed stuttering males and Downey's left handed males, or between all stutterers and all Downey's *Ss*.

*(2)* Thumbedness is not significantly related to eyedness in stutterers.

*(3)* Thumbedness is not significantly related to the hand preference dextrality quotient in stutterers.

*(4)* There is not a statistically significant difference between the proportions of stutterers who are right and left thumbed, respectively. Such difference as there is appears more conspicuous when the sexes are considered separately, there being a slight tendency toward more left thumbedness among the males, and a slightly greater tendency toward more right thumbedness among the females. This sex difference is fairly marked, but can hardly be regarded as a statistically significant difference.

*(5)* There is relatively more right thumbedness among originally right handed than among originally left handed stutterers, and the difference in this respect is almost a statistically significant one (there are 98.5 chances in 100 that it is a true difference).

*(6)* There is significantly more left than right thumbedness among originally left handed stutterers.

## Discussion

A point of special interest is that in this study, as in Downey's, there appears to be evidence of a slight sex difference, a relatively larger proportion of females than of males being right thumbed. This is of interest in connection with the greater tendency toward right hand preference among females, which has been reported by a number of writers.[7]

The findings of this study also tend to bear out Downey's conclusion that a relation exists between thumbedness and handedness (gross usage), to the extent that originally left handed stutterers present a relatively higher proportion of left than of right thumbedness. Of originally right handed stutterers, however, 50% are right and 50% are left thumbed.

With respect to the proportion who are right thumbed, stutterers fall between, and about midway between right and left handed non-stutterers. The importance of this fact depends upon the validity of Downey's classification of her *Ss*. On its face value, the finding indicates that stutterers as a group tend to be ambidextrous.

There are certain reasons, however, why this finding can hardly be accepted on its face value. Downey uses a six-fold classification of handedness: three degrees of right handedness and three degrees of left handedness. Curiously enough, considering only males, there is no appreciable difference between the stutterers and Downey's most right handed *Ss*. It is Downey's partially right handed *Ss* who differ significantly from the stutterers. Again, the stutterers differ significantly from Downey's most left handed *Ss*, and from one group of her partially left handed *Ss*,

[7] L. E. Travis, *op. cit.*, 97.

but they practically do not differ at all from her other group of partially left handed *Ss*. The rather confusing statement must be made, therefore, that with respect to thumbedness, when males only are considered, stutterers are very similar to Downey's pure right handers and one group of her partial left handers, and that they differ appreciably from her pure left handers, partial right handers, and one group of her partial left handers. If the stutterers' thumbedness is to be taken as a criterion of their handedness, as handedness is defined by Downey, it is quite impossible to conclude definitely just what the handedness of stutterers might be. It might be just as well, and possibly more legitimate, to compare all the stutterers with all of Downey's *Ss*. When this is done a significant difference is not found, as shown in Table IV.

# MUSICAL TALENT OF MEXICANS

By THOMAS R. GARTH and ETHEL CANDOR, University of Denver

In previous studies of racial differences in musical talent, the findings generally favor the assertion that no differences are found that may be called racial. Lewis and Peterson,[1] Peterson and Lanier,[2] Johnson,[3] Streep,[4] Gray and Bingham,[5] have compared negroes and whites. Garth and Isbell have studied American Indians.[6] Gray and Bingham thought they obtained measures favoring negroes in the consonance test. Garth and Isbell reported no differences due to race when they compared the performance of Indians (full and mixed bloods) with whites. Other studies of racial groups that may be mentioned are Murdock's study of Hawaiians[7] and Farnsworth's study of Chinese and Japanese.[8] In these last mentioned investigations the groups were either small or the factor of nurture was not properly controlled.

The purpose of the present study is to answer the question: "Are some races more musical than others?" In order to bring our results with Mexican children into line with those obtained from other races we have endeavored to use the tests and techniques that have previously been used.

While our larger problem was to determine whether Mexican children differ from white in performance on the musical talent tests, we were also concerned with three subsidiary problems, viz. (1) the reliability of the tests, (2) the reliability of the present measures, and (3) a comparison of the performance of the different races in the several tests—pitch, intensity, time, consonance, memory, and rhythm.

## METHOD AND PROCEDURE

*Tests.* The six original Seashore tests of musical talent were used in the present study. We did not use the revised tests because all the studies on racial differences in musical talent have been made with the original tests. Some objection concerning their reliability has been raised, but reliable coefficients have been obtained by

* Accepted for publication May 20, 1936.

[1] J. Peterson, Methods of investigating comparative abilities in races, *Ann. Amer. Acad. Pol. & Soc. Sci.*, 140, 1928, 178-185.

[2] J. Peterson and L. H. Lanier, Studies in the comparative abilities of whites and negroes, *Ment. Meas. Monog.*, 1929, (no. 5), 1-55.

[3] G. B. Johnson, A summary of negro scores on the Seashore musical talent tests, *J. Comp. Psychol.*, 11, 1931, 383-393.

[4] R. L. Streep, A comparison of white and negro children in rhythm and consonance, *J. Appl. Psychol.*, 15, 1931, 53-71.

[5] C. T. Gray and C. W. Bingham, A comparison of certain phases of musical ability of colored and white public school pupils, *J. Educ. Psychol.*, 20, 1929, 501-506.

[6] T. R. Garth and S. R. Isbell, The musical talent of Indians, *Music Super. J.*, 15, 1929, 83, 85 ff.

[7] K. M. Murdock, A study of differences found between races in intellect and morality, *School & Soc.*, 22, 1925, 628-632, 659-664.

[8] P. Farnsworth, An historical, critical, and experimental study of the Seashore-Kwalwasser test battery, *Genet. Psychol. Monog.*, 9, 1931, 291-293.

Ruch and Stoddard[9] and Highsmith[10] with whites, and by Peterson and Lanier[11] with whites and negroes.

*Subjects.* We used 746 Mexican children; 303 boys and 362 girls from the public secondary schools,[12] and 81 girls from a private school.[13] They were taken from what we would call the seventh, eighth, and ninth grades, but which are called in Mexico the first, second, and third years of the secondary schools. To the extent that the *Ss* were taken from these grades, the sampling may not be regarded as random. It is representative, however, we believe, of the secondary school population. Because of the select character of the private school, the results for the *Ss* from it are not offered in the conclusion. This reduces our number of *Ss* to 665. The tests were administered in the summer of 1932. They were given either by the junior author of this study, or by the teachers in the schools, who had been specially trained in the administration of tests.

*Treatment of the data. (1) Reliability.* We have endeavored to handle some of the data (100 cases), with a view to securing measures of reliability of the results. Here, since we were unable to test the *Ss* twice, we resorted to the method of split halves. In doing this, we used the raw scores for a test, odds against evens, and not the percentile scores. These are given for 100 cases selected at random, first as found, and then as stepped up, according to the Spearman-Brown prophecy formula (see Table I), giving the coefficient if the whole test could be correlated.

*(2) Medians.* We present also the median scores of the various tests for the several class groups for Mexican boys and for girls.

TABLE I

RELIABILITY COEFFICIENTS BY SPLIT HALVES METHOD, (100 Ss)

| | Pitch | Intensity | Time | Consonance | Memory | Rhythm |
|---|---|---|---|---|---|---|
| r | .92 | .82 | .29 | .34 | .80 | .48 |
| P.E. | .01 | .02 | .06 | .06 | .02 | .05 |
| Stepped Up r | .96 | .90 | .44 | .50 | .89 | .65 |

*(3) Overlapping.* We present the overlapping on white medians (see Table II) for six groups for which medians have been computed. Table II includes the results from the private school for girls. Table III gives the same data handled for the total group omitting those from the private school.

## RESULTS

*(1) Reliability.* The Seashore tests when administered to Mexicans are seen to be on the whole as reliable as when they were administered to whites or negroes Pitch and intensity seem to be more reliable for the Mexicans. Time and consonance are slightly worse, but the discrepancy is not great.

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[9] G. M. Ruch and G. D. Stoddard, *Tests and Measurements in High School Instruction*, 1927, 192 ff., 355-374.

[10] J. A. Highsmith, Selecting musical talent, *J. Appl. Psychol.*, 13, 1929, 486-493.

[11] Peterson and Lanier, *op. cit.*

[12] The writers are under many obligations to Miss Montana Lucia Hastings, Director of Research and Classification of the Public Schools of Mexico, for her generous assistance in this study. *Ss* were divided into groups of 50. Special test blanks printed in Spanish were used for record. The tests were given in the mornings and a short rest was allowed after every part.

[13] Collegio Sara L. Keen.

*(2) Medians.* By taking the fifty percentile as the measure of a white performance, and comparing the several Mexican medians for the categories therewith, we found that for both boys and girls the performances in pitch, intensity, consonance, and memory are inferior to that of the whites in these tests. They do better in time and rhythm, but this is merely a comparison of medians.

TABLE II

RESULTS OF THE MEASURES OF MUSICAL TALENT OF MEXICAN SCHOOL CHILDREN

| | | Public schools | | | | | Private school (Girls) | | |
|---|---|---|---|---|---|---|---|---|---|
| | | 1st year | | 2nd year | | 3rd year | | | |
| Test | | Girls | Boys | Girls | Boys | Girls | Secondary | 6th | 5th |
| Pitch | No. | 100.0 | 176.0 | 165.0 | 127.0 | 95.0 | 48.0 | 16.0 | 19.0 |
| | Med. | 39.2 | 11.6 | 21.3 | 33.8 | 21.5 | 19.3 | 47.5 | 28.8 |
| | $Q_3$ | 55.7 | 34.6 | 44.2 | 49.6 | 37.5 | 36.2 | 62.5 | 72.5 |
| | $Q_1$ | 18.3 | 5.1 | 7.7 | 12.3 | 8.8 | 10.7 | 30.0 | 7.9 |
| | % overlap. | 29.0 | 11.0 | 17.0 | 24.0 | 11.0 | 15.0 | 44.0 | 26.0 |
| Intensity | No. | 00.0 | 176.0 | 165.0 | 127.0 | 95.0 | 48.0 | 16.0 | 19.0 |
| | Med. | 145.8 | 34.1 | 30.6 | 42.3 | 35.6 | 41.4 | 40.0 | 22.5 |
| | $Q_3$ | 68.3 | 59.5 | 65.1 | 66.0 | 59.5 | 66.7 | 83.0 | 64.2 |
| | $Q_1$ | 21.4 | 12.5 | 12.2 | 14.1 | 11.9 | 8.6 | 15.0 | 7.9 |
| | % overlap. | 45.0 | 35.0 | 37.0 | 43.0 | 41.0 | 38.0 | 50.0 | 42.0 |
| Time | No. | 50.0 | 176.0 | 165.0 | 80.0 | 95.0 | 48.0 | 16.0 | 19.0 |
| | Med. | 54.3 | 43.9 | 51.7 | 49.0 | 49.5 | 68.9 | 60.0 | 41.6 |
| | $Q_3$ | 77.0 | 69.4 | 79.8 | 75.0 | 78.8 | 78.5 | 82.0 | 70.9 |
| | $Q_1$ | 21.6 | 23.0 | 24.0 | 26.6 | 31.8 | 14.7 | 40.0 | 13.6 |
| | % overlap. | 56.0 | 42.0 | 52.0 | 48.0 | 50.0 | 77.0 | 63.0 | 37.0 |
| Consonance | No. | 100.0 | 176.0 | 165.0 | 127.0 | 95.0 | 48.0 | 16.0 | 19.0 |
| | Med. | 42.8 | 36.4 | 35.1 | 27.4 | 43.1 | 32.7 | 64.3 | 58.8 |
| | $Q_3$ | 64.4 | 58.3 | 59.8 | 52.5 | 73.6 | 50.0 | 70.0 | 81.3 |
| | $Q_1$ | 23.0 | 20.0 | 19.4 | 15.9 | 23.1 | 14.7 | 46.6 | 43.8 |
| | % overlap. | 40.0 | 32.0 | 34.0 | 28.0 | 44.0 | 25.0 | 69.0 | 68.0 |
| Memory | No. | 100.0 | 176.0 | 164.0 | 127.0 | 95.0 | 48.0 | 16.0 | 19.0 |
| | Med. | 40.0 | 27.3 | 46.1 | 29.2 | 23.9 | 67.1 | 80.0 | 38.3 |
| | $Q_3$ | 65.0 | 56.1 | 68.8 | 57.3 | 55.3 | 85.0 | 92.0 | 70.8 |
| | $Q_1$ | 20.0 | 8.1 | 19.3 | 15.2 | 10.4 | 30.0 | 40.0 | 17.5 |
| | % overlap. | 38.0 | 29.0 | 48.0 | 33.0 | 30.0 | 63.0 | 75.0 | 42.0 |
| Rhythm | No. | 00.0 | 176.0 | 164.0 | 127.0 | 95.0 | 48.0 | 16.0 | 19.0 |
| | Med. | 184.0 | 74.2 | 78.7 | 75.6 | 72.5 | 86.0 | 80.0 | 45.0 |
| | $Q_3$ | 94.0 | 88.8 | 89.7 | 91.1 | 88.5 | 96.7 | 86.6 | 71.25 |
| | $Q_1$ | 48.0 | 48.8 | 57.0 | 52.9 | 47.9 | 68.3 | 53.3 | 9.5 |
| | % overlap. | 74.0 | 75.0 | 79.0 | 76.0 | 74.0 | 94.0 | 81.0 | 47.0 |

*(3) Overlapping.* If we compare the overlapping of Mexican performance on white medians, the situation is more favorable for the Mexicans, and points to similarity, or tendency thereto, of performance with the exceptions of pitch and rhythm. In the former they are inferior, and in the latter they are superior, in their performance. To be sure, the comparison of the total groups is more instructive, but here the Mexicans all seem to be inferior in pitch, the overlapping for

boys 16% and for girls 19%, and superior in rhythm, overlapping being for boys 75% and girls 76%. In no other cases does the overlapping reveal out-and-out differences between whites and Mexicans, for usually the overlapping hovers around 50%. It is not known why these Mexican children are not as good as whites in their performance on the test for pitch. We suspect it is due to their inexperience in taking tests. We do not know why they do so well in rhythm, but feel it may be due to experience in dancing. Therefore, we do not feel that these differences as found are due to race, but to nurture.

TABLE III

RESULTS OF THE MEASURES OF MUSICAL TALENT OF 665 PUBLIC SCHOOL CHILDREN OF MEXICO

| | Pitch | | Intensity | | Time | |
|---|---|---|---|---|---|---|
| | Boys | Girls | Boys | Girls | Boys | Girls |
| No. | 303.0 | 360.0 | 303.0 | 360.0 | 255.0 | 310.0 |
| Med. | 19.8 | 25.6 | 37.3 | 37.8 | 45.3 | 51.8 |
| $Q_3$ | 43.0 | 45.8 | 62.7 | 64.5 | 71.3 | 74.8 |
| $Q_1$ | 6.7 | 9.0 | 13.2 | 14.4 | 23.7 | 27.3 |
| % overlap. | 16.0 | 19.0 | 38.0 | 40.0 | 44.0 | 52.0 |

| | Consonance | | Memory | | Rhythm | |
|---|---|---|---|---|---|---|
| | Boys | Girls | Boys | Girls | Boys | Girls |
| No. | 303.0 | 360.0 | 303.0 | 359.0 | 303.0 | 359.0 |
| Med. | 33.9 | 38.1 | 28.1 | 40.4 | 74.8 | 78.1 |
| $Q_3$ | 56.1 | 66.1 | 56.8 | 65.2 | 89.9 | 91.1 |
| $Q_1$ | 18.0 | 21.2 | 11.3 | 15.7 | 50.3 | 52.2 |
| % overlap. | 30.0 | 38.0 | 31.0 | 39.0 | 75.0 | 76.0 |

If we correct our findings for these factors of nurture, we shall have to say that these tests do not give evidence for racial differences in musical talent as between whites and Mexicans.

## SUMMARY

The present study indicates superficial differences between Mexicans and whites in pitch and in rhythm. The Mexicans are inferior in the former and superior in the latter. Shall we take these indicated differences as racial differences?

In the light of the fact that successful performance in the Pitch Test involves apprehending the physiological limit of the student, we doubt if the measures here are fair for the Mexican children. A successful performance involves more experience possibly than was possessed by these children. The superiority of the whites is due probably to their superior experience in such procedures as are involved in taking the test.

As to rhythm, perhaps the Mexicans' experience in the dance is responsible for their superior performance in this test.

## MINOR STUDIES FROM THE PSYCHOLOGICAL LABORATORY OF CORNELL UNIVERSITY

### LXXXIII. The Limen of Heat and Some Conditions Affecting It

By Eleanor Lowenstein and Karl M. Dallenbach

In an earlier study,[1] we determined, with 100 *Os*, the extent and frequency of dispersion of the critical stimulus-values for heat and for burning or painful heat. Beginning with a temperature that felt slightly warm, we slowly increased the temperature by successive steps until *O* reported 'heat' and 'burning heat.' From this single ascending series of trials we obtained 'limens' for heat and burning heat from every one of our *Os*.[2] Individual differences were large, the critical stimulus for heat varying from 40°–46° C. (average 42.89 ± 1.07°), and for burning heat from 43°–51° (average 47.09 ± 1.29°).[3]

While these results gave us a range of temperatures for a group of *Os*,[4] they told us nothing regarding the constancy of the critical temperature for a given *O*.

*Object.* In the present study we set out to determine for a few *Os* the limens of heat, leaving for future investigation the problem of burning heat. We wished, in addition, to isolate and identify some of the factors upon which sensitivity depends.

#### Method and Procedure

*Apparatus.* We used the same apparatus as in the earlier study: a single-system temperature-grill made of small copper tubing.[5] This stimulator was attached to the copper coil of a variable constant-temperature bath, and the coil was in turn connected to a faucet leading from the University water mains. The stimulus-temperature could, therefore, be controlled in two ways: by varying the temperature of the water bath, and by altering the water's rate of flow through the grill system.

The temperature of the water passing through the system was read with a magnifying glass from a 1/10-degree centigrade thermometer just before the water entered the grill and just after it emerged. The average of the two readings was taken as the stimulus-temperature.

*Observers.* The *Os* were Mr. Frank Locke (*L*), Mr. George Pavliceck (*P*), and Miss Edith Varon (*V*), senior undergraduates majoring in psychology. Though all experienced observers, they were untrained in the observation of warmth and heat. Their knowledge of the problem was limited to what was furnished them in the instructions.

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* Accepted for publication August 22, 1935.

[1] Eleanor Lowenstein and K. M. Dallenbach, The critical temperatures for heat and for burning heat, this Journal, 42, 1930, 423-429.

[2] Though this procedure is, as we pointed out (*op. cit.*, 425 f.) open to obvious criticism, it sufficed for our purpose. Our study was not designed to obtain precise, statistically computed limens, but simply ranges of adequate temperatures.

[3] *Op. cit.*, 425 f.

[4] They also explain why different experimenters obtained discrepant results with similar stimulus-temperatures. A given temperature may feel 'warm' to one *O*, 'hot' to another *O*, and 'burning hot' to a third.

[5] *Op. cit.*, 424.

*Instructions.* The following instructions were given the *Os* before every series of experiments.

This series will be in ascending (descending) order. At the signal 'ready,' hold your forearm above the grill. At 'now,' place the volar surface upon the grill. Relax and keep the pressure of your arm as constant and as uniform as possible. At a second 'now,' remove your arm and describe your experience.

After the first trial in a series, *E* merely said, 'Ready, now.'

*Procedure.* The serial order of the psychophysical Method of Limits was used. *O* sat in a large easy chair with his right arm parallel to the grill, which rested upon a table beside him. At the signal "ready," he raised his right arm above the grill and at "now," placed the volar surface upon it; 5 sec. later, at the second "now," he removed his arm and gave his report. After an interval of about 100 sec. during which the temperature of the grill was raised 1°, the signal 'ready,'—'now' was again given, and again *O* rested his arm upon the stimulator. This procedure was repeated, with stimulus increments of 1°, until *O* reported heat. Then, after a rest of several minutes during which the temperature of the grill was raised from 6–10°, there followed a descending series of trials, moving from heat to warmth.

The series were alternated in this manner, so as to make an even number (2, 4, or 6) during a single observational hour. The length of a series varied from 3 to 9 steps.

Every *O* was given 20 preliminary series (10 ascending and 10 descending) to acquaint him with the procedure and to aid him in establishing his criteria of report. He was then given the 50 experimental series (25 ascending and 25 descending) which supplied the data of this study.

The temperature of the skin was taken at the beginning and at the end of every series by means of a delicate centigrade thermometer calibrated in 1/10°. The thermometer was placed on a thick pad of cotton. Laying the experimental surface of his forearm over the thermometer, *O* kept his forearm in position until the mercury column ceased to rise, and the thermometer was then read, the average of the readings before and after a series was regarded as the skin-temperature during that series.

The temperature of the experimental room was also taken at the beginning and at the end of every series. The range of variation was large, since the experiment began late in winter and continued through spring to early summer.

## Results

*(1) Limens.* The limens for heat obtained in this study are given in Table I. They vary slightly for the different *Os*: *P*, 40.66°; *V*, 41.56°; *L*, 42.33°. All, however, fall within the lower half of the frequency curve that was obtained in the earlier study with 100 *Os*, and are lower than any previously reported.[6] These results not only imply that our *Os* were unusually sensitive to heat, but also demonstrate once more the need for care in selecting stimuli that fall within the temperature range 40°–45°.[7] A temperature within that range may yield warmth for one *O* and heat for another.

*(2) Dispersion.* As the S.D. and the ranges of dispersion given in Table I show, the limens of heat vary like other liminal determinations. There is no single critical

[6] Cf. Lowenstein and Dallenbach (*op. cit.*, 423 f.) for references to the earlier work.
[7] *Idem.*, 428.

temperature, as is frequently suggested in textbooks, at which the experience of heat always emerges. Our *O*s' reports of heat are scattered among the different stimulus-temperatures. Not only do temperatures within the critical range 40°–45° affect each *O* differently, but they also affect the same *O* differently at different times.

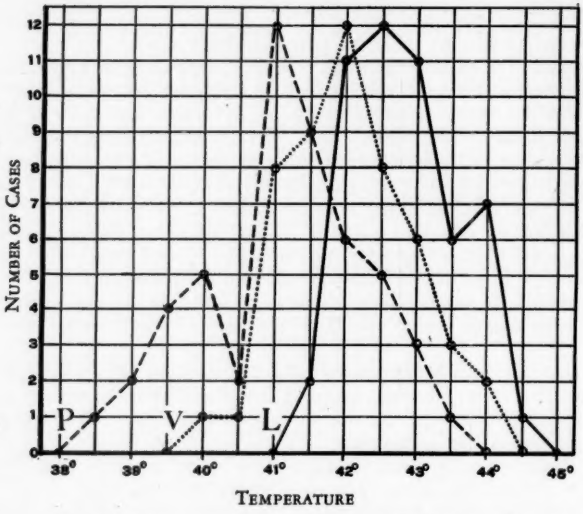


FIG. 1. FREQUENCY CURVES SHOWING DISPERSION OF REPORTS OF HEAT

Ascending and descending series give closely corresponding ranges of dispersion and the averages differ only by +0.02° for *V*, +0.01° for *L*, and —0.4° for *P*. These results indicate that our efforts to avoid adaptation and fatigue were successful, or

TABLE I

LIMENS, AVERAGES OF ASCENDING AND DESCENDING SERIES, STANDARD DEVIATIONS, AND RANGES OF DISPERSION

| *O* | Limens | | | Ascending series (1st report of heat) | | | Descending series (1st report of warmth) | | |
|---|---|---|---|---|---|---|---|---|---|
| | L | S.D. | range | Av. | S.D. | range | Av. | S.D. | Range |
| *L* | 42.33° | .75° | 41–44.5° | 42.38° | .53° | 41–44.5° | 42.28° | .76° | 41–43.5° |
| *P* | 40.66° | 1.44° | 38–43.5° | 40.46° | .86° | 39–43.5° | 40.86° | 1.04° | 38–42.5° |
| *V* | 41.56° | .87° | 39.5–44.0° | 41.57° | .84° | 40.5–44.0° | 41.55° | .82° | 39.5–43.0° |

at least that we succeeded in reducing these factors to a common denominator in the ascending and descending series.

The frequency of reports of heat is plotted for every *O* in Fig. 1 against stimulus-temperature. Temperatures at the first report of heat in the ascending series and of

the last report in the descending series were used in the construction of the curves.[8] These curves show that the range of dispersion is greatest for *P* (5°), who has the lowest limen; intermediate for *V* (4°), who has a limen of intermediate size; and smallest for *L* (3°), who has the highest limen.

*(3) Skin-temperature.* The temperature of the experimental area varied considerably, as shown in Table II, for all the *O*s. The variations, however, are not significantly related to the stimulus-conditions of our experiment. This is shown by the large dispersion and overlap (as expressed by the S.D.) of the skin-temperatures taken at the end of the ascending and descending series, and also by the small differences between the average temperatures of the two arrays (+0.13°, +0.2°, and +0.15° for *L*, *P*, and *V* respectively). Short stimulus-durations (5 sec.) and long rests (100 sec. or more) between stimulations sufficed apparently to leave skin-temperature unaffected.

TABLE II

SKIN-TEMPERATURE DURING THE ENTIRE EXPERIMENT AND AT END OF ASCENDING AND DESCENDING SERIES

| *O* | During entire experiment | | | End of ascending series | | End of descending series | | Difference between descending and ascending series |
|---|---|---|---|---|---|---|---|---|
| | Av. | S.D. | range | Av. | S.D. | Av. | S.D. | |
| *L* | 35.36° | .52° | 34.1–36.2° | 35.29° | .54° | 35.42° | .49° | +.13° |
| *P* | 34.77° | .55° | 33.0–36.1° | 34.67° | .52° | 34.87° | .50° | +.20° |
| *V* | 33.54° | .88° | 31.2–35.4° | 33.47° | .94° | 33.62° | .80° | +.15° |

TABLE III

CORRELATIONS OF TEMPERATURE OF STIMULUS AND SKIN, STIMULUS AND ROOM, AND SKIN AND ROOM

| *O* | Stimulus- and skin-temperature | | Stimulus- and room-temperature | | Skin- and room-temperature | |
|---|---|---|---|---|---|---|
| | r | P. E. | r | P. E. | r | P. E. |
| *L* | −.078 | .094 | +.621 | .065 | +.784 | .041 |
| *P* | +.498 | .071 | +.368 | .079 | +.695 | .053 |
| *V* | +.491 | .072 | +.371 | .085 | +.542 | .069 |

In an endeavor to discover whether variations in reports of heat were in any way conditioned by skin-temperature, we drew correlations, by the product-moments method, between the average skin-temperature during a series and the critical stimulus-temperature of that same series. The coefficients with their P.E. given in Table III, indicate that skin-temperature was ineffective for *L* and only slightly effective for *P* and *V*. It should be noted, however, that *L* showed the smallest variation in skin-temperature (2.1° as opposed to 3.1° and 4.2°) and the smallest difference between ascending and descending series. We may conclude from these results that the limen of heat is not greatly affected by slight deviations in the skin-temperature.

[8] The limens given in Table I were not computed in this fashion but, in accordance with the Method of Limits, by averaging the first reports of heat in the ascending series with the first reports of warmth in the descending series. Averages of the values plotted in Fig. 1, will give a result ½° too high, since the values of the 25 descending series are transposed 1° upwards to the last report of heat.

*(4) Room-temperature.* The temperature of the experimental room was taken, it will be recalled, at the beginning and end of every series. The average of the two readings was correlated, by the products-moments method, with the critical stimulus-temperature in that series. The coefficients, given in the second section of Table III, are positive for all the *O*s. Though the correlation is low for *P* and *V*, it is high enough for *L* to indicate that for him at least room-temperature was a positive factor: the higher the room-temperature the higher must be the temperature of the stimulus to arouse heat.[9]

*(5) Skin-temperature vs. room-temperature.* Our data enabled us to draw a correlation between skin- and room-temperature. The coefficients, given with their P.E. in the last section of Table III, are, as was expected, positive and comparatively high. During good health (which our *O*s enjoyed during the course of this study) and under stable conditions of humidity (not controlled by us) and of air-movement (only partially controlled), the temperature of the exposed areas of the skin varies with the temperature of the surrounding air.

In the intercorrelation of our three variables, *P* and *V* give consistent results and stand in close agreement with one another. *L*'s results, however, are inconsistent, yielding a high positive correlation between skin-temperature and room-temperature and between room-temperature and report of heat, but not between skin-temperature and report of heat. It may be that his variation in skin-temperature, the smallest among our *O*s, was large enough to correlate with room-temperature, but was too small to correlate with report of heat. Beyond this possibility, we have no explanation for this inconsistency.

## Summary

Limens of heat were obtained by the Method of Limits from three *O*s. The *O*s reported heat at temperatures varying from 39° to 44.5°. Their averages were: *L*, 42.33°, P.E., 0.75°; *V*, 41.56°, P.E. 0.87°; *P*, 40.66°, P.E. 1.44°. The effectiveness of stimulus-temperature was conditioned by room-temperature and, to a slight degree, by skin-temperature. Room- and skin-temperatures, under the conditions of our experiment, were closely correlated.

[9] These results clearly indicate the need of a constant temperature-room for experiments of this kind.

# APPARATUS

## A NEW POLYGRAPH

By HAROLD A. KOHN, Mooseheart Laboratory for Child Research

The chief advantages of the polygraph described here over others now on the market are as follows.[1] (1) No external batteries are required for its operation; all the power is taken directly from the 110-volt lighting-circuit. (2) It is extremely sensitive; even the most rapid tapping rates will be accurately recorded by it. (3) It is compact and portable, being provided with a case and about the size of a small typewriter. (4) Standard fountain pens and pencils, may be used for writing-points without making any changes in the apparatus whatsoever. (5) The pressure and amplitude of the writing points may be regulated independently. (6) The apparatus is extremely simple to load with paper of which it carries a large supply—a roll 5 in. wide, 450 ft. long. (7) The speed at which the record paper is moved under the writing-points is widely variable and is governed by a rheostat. It is not necessary to shift gears to pass from one speed to another. (8) The movement of the paper may be arrested at any point without stopping the motor, simply by turning a thumb set. (9) All adjustments—pressure and amplitude of the writing-points, and speed and tension of the record-paper—may be easily made, and after once being made they require no further attention during the period of experimentation.

This polygraph is equipped with three writing-points. The electro-magnets which operate them are actuated through a 24-volt transformer attached directly to the 110-volt lighting-circuit. As each individual magnet is actuated, the action is magnified and transmitted to its corresponding shoe by means of a system of levers. Each shoe, in turn, is brought into contact with a revolving drum which, through another series of levers, magnifies this excursion-action of the shoe, transmits it to the guide wire attached to the proper pen receptacle and pulls the pen or pencil quickly across the surface of the paper. The amplitude of the pen stroke is a function of the amplitude of the shoe movement. Thus, by controlling the excursion of each shoe individually, by means of adjustable stop screws, the amplitude of the individual pens is regulated. The surface of the shoe which comes into contact with the drum must describe a curve, the absolute center of which is the pivot for the lever system.

A 1/20 h.p., variable speed, governor-controlled-motor, powers the polygraph. This motor is of such construction and is so cradled that no vibration is evident. The motor speed, 1750 r.p.m., is reduced in a 6 to 1 ratio by means of a built-in gear. This is further reduced in an 84 to 1 ratio by means of an external gear. The Jefferson step-down transformer set for 24 volts, serves to supply the three magnets. As the electrical contact is broken, the shoe is returned to its normal position through the demagnetization, and a powerful spring returns the pen instantly to the starting position. The load of the shoe upon the drum is taken up by

[1] This apparatus was constructed under the direction of Dr. M. L. Reymert.

the governor, resulting in a smooth action of the paper through the polygraph at any speed.

The machine is loaded by loosening paper tension screws (12), lifting the paper reel up and inserting the new roll, tightening the tension screws slightly again. The set screw for lifting the support is loosened and the entire pen support raised to a vertical position. The paper is now threaded under the support and the screw (4)

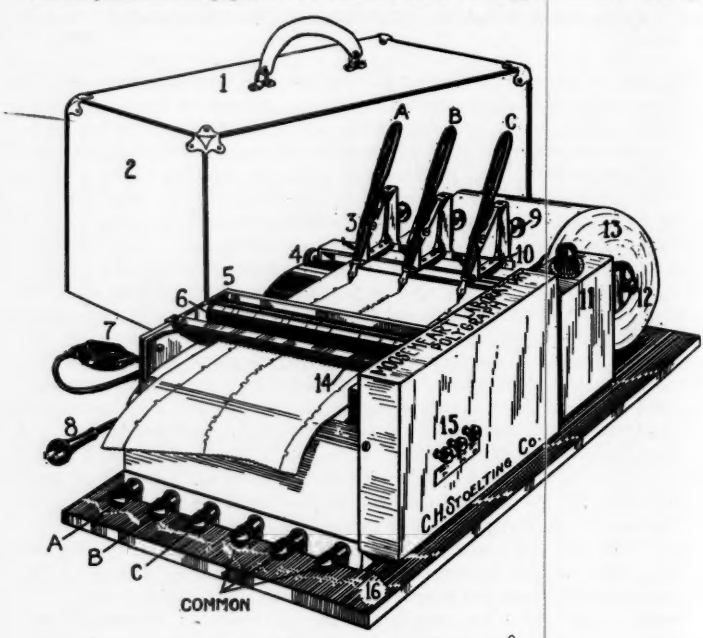


FIG. 1. THE PORTABLE POLYGRAPH

(1) Pens A, B, C, carried under lid
(2) Finished wood case
(3) Adjustable clamp pen holder
(4) Set screw for lifting support
(5) Hinged roller casing, lifts forward
(6) Idle roller
(7) Cord switch
(8) Plug to 110-volt circuit
(9) Pressure screws for pencil recording
(10) Pen swivels
(11) Rheostat with calibrated dial
(12) Paper tension screws
(13) 450-ft. paper reel
(14) Paper cutting edge
(15) Amplitude regulation screws
(16) Wood base, mounted on rubber feet

is retightened. Hinged roller casing (5) is now lifted forward and the paper placed between the feed and idle rollers; the casing is dropped back into place. The paper reel is now ready to be run through the machine. The pens are inserted in the proper holders (3) and the clamp tightened so as to hold the pens securely in place. For pen records the pressure screws (9) are released as far as possible. These are to

be used only in pencil recording. This machine is now ready for operation. After plugging into a light socket, the switch (7) is thrown on; a slight hum will now be heard. The rheostat (11) is set for any speed desired and the paper begins to wind through the machine. When the circuit between any of the binding posts, A, B, C, and their common binding posts is closed, each pen will immediately be individually actuated. The amplitude regulators (15) may now be set for the pen excursion desired. The model is designed especially for types of experimental situations involving a single stimulus, a single response, and a constant time line. Servicing the polygraph with any desired number of pen receptacles in excess of three involves only slight changes in construction.

Simple timing devices, such as are available in all psychological laboratories, will serve to actuate the constant-time pen. If a mercury contact metronome or any other similar type of electrical timing instrument is used, there will, of course, be no need for independent batteries as the magnets and their binding posts are already serviced with power.

## APPARATUS NOTES

### A One-Way Vision Screen

Black cellophane has been found to make an excellent one-way vision screen. The one-way effect is secured with relatively little difference in illumination on the two sides of the screen. The field observed through the screen is somewhat darkened, and is slightly streaked by the particular brand of cellophane which has been tried. The advantages which this material offers are its 100% one-way effect, low cost, and the ease with which it may be used.[1] The particular fault is the likelihood of tearing. This fault is somewhat overcome by attaching the cellophane with thumbtacks placed at frequent intervals. If desired, further protection could be obtained by using plate glass on one or both sides.

A screen of this material is easily used with naïve adult human subjects without its presence being suspected. The jet-black shade makes the cellophane appear to be opaque to ordinary light.

Columbia University J. G. Pratt

[1] The cellophane which has been used is packed and distributed by the Dennison Manufacturing Co., Framingham, Mass. It may be purchased from Gimbels, New York City, at 25 cents for a roll 20 in. by 150 in.

# NOTES AND DISCUSSIONS

## Mind, Mechanism, and Lexicographic Behavior

In his presidential address at the Dartmouth meeting of the American Psychological Association last September,[1] Professor Clark L. Hull called attention to the "paradox of Eddington, the physicist, apparently insisting that the higher forms of behavior are at bottom non-physical, whereas Weiss, the psychologist, insists that they are fundamentally non-psychological. . . . Eddington seems to be implying that we *can not* reach a sound theory of rational, purposive and moral behavior if we set out with nothing but aether and electrons. Weiss is saying, by implication, that a sound theory of such behavior *can* be reached by setting out with nothing but electrons and protons."[2]

After a brief discussion of the methodology of scientific theory, Professor Hull goes on to say, "It becomes quite clear, for example, what Weiss and Eddington should have done to substantiate their claims. They should have exhibited, as strict logical deductions from explicitly stated postulates, a series of theorems corresponding in detail to the concrete manifestations of the higher forms of human behavior. Then, and only then, they might proceed to the examination of the postulates of such a system."[3]

Further on, he continues, "but if neither Weiss nor Eddington, nor any other writer in this field, has been able to bring forward the indispensable systematic theory as a prerequisite of the logical right to express a valid conclusion concerning the ultimate nature of higher adaptive behavior, may this not mean that the attainment of such a system is impossible, and that, consequently, the problem still remains in the realm of philosophical speculation? There is reason to believe that this is not the case. The ground for optimism lies in part in the small theoretical system which is presented below (p. 15)."[4]

Obviously the first requisite of a scientific system is intelligibility. To insure this, Professor Hull begins his system with a set of eighteen definitions of the critical terms to be employed in his postulates and theorems. In the interests of intelligibility, it would have seemed desirable to use common English words in their most usual meanings, and to invent if necessary a few new technical terms for those concepts for which there is no ordinary English equivalent. Instead, Professor Hull selects eighteen ordinary English words and phrases and assigns to them meanings astonishing to the lexicographer. For example, an *attempt* is defined as "a segment of behavior the termination of which is marked by either reinforcement or extinction."[5] It would be hard to invent a more perfect example of defining the known in terms of the unknown. Why does he do it?

In the latter part of his address, Professor Hull implies that while he is not seeking to deny the existence of consciousness, he is not willing to admit its primacy in determining behavior.[6] Now since no contemporary psychologist contends that

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[1] C. L. Hull, Mind, mechanism, and adaptive behavior, *Psychol. Rev.*, 44, 1937, 1-32.

[2] *Ibid.*, 4. [3] *Ibid.*, 9. [4] *Ibid.*, 10-11.

[5] *Ibid.*, 15, def. 5.

[6] "What then shall we say about consciousness? Is its existence denied? By no

consciousness determines *all* behavior, the only possible argument concerns the question as to whether it determines *any* behavior. Does Professor Hull intend to construct a hypothetical system in which *all* behavior is explained without any reference to mind?

From his criticism of Eddington, we should infer that he does, for Eddington was perfectly willing to admit that much behavior, even much human behavior, might conceivably be explained in physical terms. Eddington merely contended that physical knowledge was insufficient to explain "rational man morally responsible." Is this what Professor Hull intends to explain by his new scientific system?

It is a difficult undertaking. In fact, to explain the simplest forms of animal behavior in purely mechanistic terms is sufficiently difficult for anyone. Many psychologists who have sought a purely mechanistic interpretation of behavior have reported their own experimental results in terms shot through with implications of mind and purpose. The systematic psychologist who would make any reference to experimental results, even those of pure behaviorists, can scarcely dispense with such terms as "attempt," "trial-and-error," "striving," "goal," "failure," for these are the terms in which the observed phenomena have been described. He who would explain these phenomena mechanistically must keep enough of the accepted vocabulary so that his readers may know to what phenomena he is referring. If mind and purpose have no place in his system, the teleological words and phrases must be redefined in mechanical terms. Apparently this is what Professor Hull has set out to do.

Those of us who have been in the business of writing definitions and of examining critically the definitions written by other psychologists are well aware of the difficulties involved. Terms can be defined only by using other terms. Unless these other terms are simple and unequivocal, they in turn must be defined, and in the second set of definitions the original terms must be rigorously avoided. Definition in a circle is the unpardonable sin in precise logical exposition. So after one has used a third set of terms to define the second, one will require a fourth set to define the third, and so on. In avoiding the vicious circle, one risks the parabola of definitions, stretching toward infinity at both ends. Even after the number of definitions runs into the thousands, one is no better off. The only way to break the endless chain of definitions is to define at least some of our basic scientific concepts in terms so simple so unequivocal, so familiar, and so universally understood that they need no further definition. Therefore, it is no criticism of Professor Hull's system to point out that he uses many terms, such as "drive" and "hunger" which he does not attempt to define. It is unfortunate, however, that in common English usage, some of the undefined terms belong to the teleological system of interpretation from which Professor Hull is trying to free scientific thinking.

Scarcely less confusing is the fact that in constructing the vocabulary which he does define, Professor Hull has found it necessary to assign to simple, everyday words meanings so different from those familiar to most readers. The word "discouragement" is not absolutely indispensible, so far as its prevalence in psychological literature is concerned. It is in a very different category from such expressions as "trial-and-error." It would have been less perplexing and more appropriate to invent a nonsense syllable for the meaning which Professor Hull assigns to *discouragement.*

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means. But to recognize the existence of a phenomenon is not the same thing as insisting upon its basic, *i.e.* logical, priority." (*Ibid.*, 30.)

"*Discouragement* is the diminution in the power of one excitatory tendency to evoke its normal reaction, this diminution resulting from one or more unsuccessful attempts involving a second reaction."[7] In first reading this definition, one feels impelled to ask what Professor Hull means by *unsuccessful*. If an attempt is only "a segment of behavior the termination of which is marked by either reinforcement or extinction," how can it be either successful or unsuccessful?

If the reader is one of those conscientious souls who must understand each sentence before proceeding to the next, he will never get beyond "discouragement," but if he is willing to skim back and forth, gradually piecing the puzzle together, he will discover from definition 14 that "*success* is the culmination of striving which is characterized by the occurrence of the full reaction ($R_G$) of which the fractional anticipatory component ($r_G$) is a part."[8]

What is a "full reaction?" Professor Hull does not define it. By what criteria are we to judge its fullness? And what is a "fractional anticipatory component?" "Striving" and "anticipatory" both have teleological connotations, but glancing back up the page, we note that Professor Hull has defined *striving* as "that behavior of organisms which, upon frustration, displays varied alternative action sequences, all *directed* by an intent ($r_G$) to the attainment of the same reinforcing state of affairs."[9] In ordinary English, *intent* means *purpose*. In logic, it may mean a *concept* or *notion*, and it has a few other special meanings, all of which refer to *mind*. What place can *intent* have in a theoretical system that seeks to account for behavior mechanistically? But perhaps Professor Hull has given the word *intent* a brand new meaning, just as he has done for so many other common English words. We note that intent is followed by the mysterious symbol ($r_G$). Now a symbol of this sort, if it is used at all, should have one and only one precise meaning throughout the entire system. If we can discover what ($r_G$) means, perhaps we shall begin to see some meaning in Professor Hull's definitions of *striving*, *goal*, *anticipate*, *success*, *failure*, and *disappointment*, for the symbol ($r_G$) occurs in each of these definitions. Under the heading *Key to Diagrams*, we find the equation "$r_G$ = a fractional component of the goal reaction."[10] What is a goal reaction? Professor Hull defines a *goal* as "the reinforcing state of affairs towards the attainment of which a behavior sequence of an organism may be directed by its intent ($r_G$)."[11] Since *goal* is thus defined in terms of $r_G$, the equation "$r_G$ = a fractional component of the goal reaction" cannot be intended as a definition of $r_G$. If it were, we should have an example of a vicious circle, and of course that is not to be thought of. We must look elsewhere to discover what $r_G$ means.

Let us examine the definition in which the mysterious symbol first occurs, Definition 10. At first glance, it appears to be a definition of the word *directed*, for that word is italicized, but perhaps it may throw some light on the meaning of $r_G$. "10. A behavior sequence is said to be *directed* to the attainment of a particular state of affairs when there appears throughout the sequence a characteristic component ($r_G$) of the action ($R_G$) closely associated with the state of affairs in question and this component action ($r_G$) as a stimulus tends to evoke an action sequence leading to the total reaction ($R_G$) of which the component constitutes a part."[12]

What is a "characteristic component" of an action? It might be the movement of

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[7] *Ibid.*, 16. [8] *Ibid.*, 16, def. 14. [9] *Ibid.*, 16, def. 11.
[10] *Ibid.*, 17. [11] *Ibid.*, 16, def., 12. [12] *Ibid.*, 16, def. 10.

bones, or the contraction of muscles, or some chemical change, such as the formation of lactic acid, or a change in the electrical potential of nerves, muscles, and other tissues. The possibilities are practically infinite. Mechanical, thermal, chemical, and electrical changes appear throughout any behavior sequence, and any one of them may be regarded as characteristic, according to the particular scientific interests of the scientist describing the phenomena. Furthermore, any one of these changes occuring in connection with an action might (theoretically, for all we know to the contrary) act as a stimulus, tending "to evoke an action sequence leading to the total reaction," assuming that there is such a thing as a "total reaction." Definition 10 is so general and vague that it might mean almost anything, which is another way of saying that it means nothing.

Disregarding for a moment the ambiguities in Professor Hull's definitions, and considering only his postulates and theorems, we may readily admit they present a logically coherent theory of a machine interacting with its environment. In a passage previously quoted by Professor Hull, Eddington had implicitly conceded the possibility of a physical theory of this kind.

"Conceivably we might reach a human machine interacting by reflexes with its environment; but we cannot reach rational man morally responsible."[13]

What has Professor Hull's theoretical system to say about "rational man, morally responsible"? Nothing at all. From the first definitions and postulates down through the last theorem, we discover no allusion to such a being.

We remember that Professor Hull took Weiss and Eddington to task for failing to exhibit a "series of theorems corresponding in detail to the concrete manifestations of the higher forms of human behavior." Professor Hull has given us a series of theorems, but among them we search in vain for any reference to the "higher forms of human behavior." So far as Professor Hull's contribution is concerned, these remain precisely where he criticized Weiss and Eddington for leaving them, "in the realm of philosophical speculation." This being the case, we wonder why Professor Hull has gone to such extraordinary trouble to rewrite the English language.

West Springfield, Mass. MABEL F. MARTIN

## THE BEZOLD-BRÜCKE PHENOMENON AND CONTOURS FOR CONSTANT HUE

Spectral hue is a joint function of wave-length and intensity. If wave-length is kept constant, hue varies with intensity except at the three invariable points in Y, G, and B. This variation is the Bezold-Brücke phenomenon. The phenomenon can be measured by determining how much the wave-length must be altered to keep hue unchanged when intensity changes. From such data it is possible to plot, against wave-length and intensity, contours for constant hue, such as are shown in Fig. 1. This figure exhibits the Bezold-Brücke phenomenon especially clearly in that it shows how the contours converge upon the invariable Y and the invariable B when intensity decreases, and upon the invariable G (and, outside the figure, the invariable non-spectral R) when intensity increases. In considering this figure, the reader must remember that the contours, in order to maintain hue constant, deviate in the direction opposite to the direction in which hue tends to change. For instance, in the YG region with decreasing intensity, the contour approaches Y in order to keep

[13] Quoted by Professor Hull, *ibid.*, p. 3.

TABLE I

BEZOLD-BRÜCKE PHENOMENON

The table shows the increment or decrement in wave-length (millimicrons) that is required to maintain constancy of hue in the face of the intensitive decrement (photons) listed at the top of each column.

| Wave-length of brighter stimulus | Intensitive decrement in photons | | | | | |
|---|---|---|---|---|---|---|
| | 2000 to 400 | 2000 to 100 | 1000 to 100 | 400 to 100 | 400 to 20 | 100 to 10 |
| 660 | −23 | −34 | −31 | −15 | — | — |
| 650 | −19 | −26 | −26 | −13 | −24 | — |
| 625 | − 5 | −11 | −14 | − 8 | −12 | −11 |
| 600 | − 3 | − 6 | − 6 | − 4 | − 7 | − 4 |
| 575 | − 1 | − 2 | 0 | + 1 | − 1 | − 1 |
| 550 | + 7 | + 8 | + 5 | + 5 | + 6 | + 2 |
| 525 | +11 | +21 | +20 | + 8 | +18 | + 4 |
| 505 | + 2 | − 2 | − 2 | + 3 | 0 | + 4 |
| 500 | − 2 | − 4 | − 4 | 0 | − 3 | 0 |
| 490 | − 2 | − 3 | − 3 | − 1 | − 3 | − 2 |
| 475 | + 2 | 0 | 0 | + 1 | − 2 | 0 |
| 460 | + 2 | + 3 | + 5 | + 1 | + 2 | + 6 |

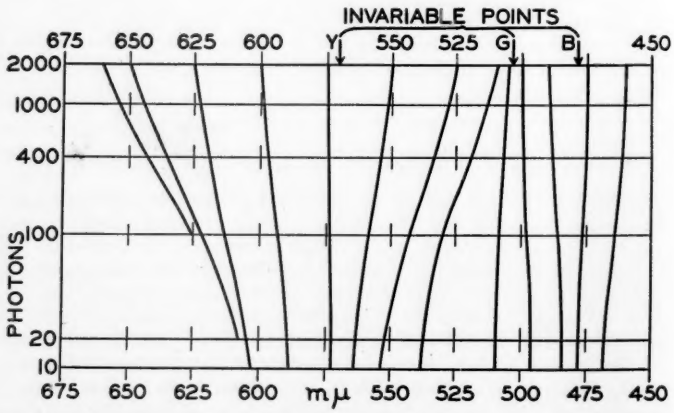


FIG. 1. BEZOLD-BRÜCKE PHENOMENON AND CONTOURS FOR CONSTANT HUE

The figure shows contours for constant hue plotted against stimulus-intensity (photons) and wave-length (millimicrons). The contours converge upon or diverge from the three spectral invariable points.

hue constant, because with wave-length constant the hue would approach G.

The data are from my thesis[1] and have not heretofore been published in full.[2] Table I gives the fundamental data. It shows for 12 wave-lengths and for 6 stated intensitive decreases (expressed in photons) what change of wave-length, positive or negative, is necessary to maintain constant hue. The curves of Fig. 1 are constructed from the curves implied by Table I. The curves in Fig. 1 have been smoothed slightly so that the figure may show the generalization of the Bezold-Brücke phenomenon, but surprisingly little smoothing has been necessary, as any one will find who will replot them from the data of Table I.

Consideration of the family of curves in Fig. 1 seems to indicate that the invariable points lie at 478, 503, and 572 mμ, instead of 474, 506, and 571 mμ as I previously estimated.[3] These differences, however, are probably not significant as compared with the individual differences that affect different determinations.[4]

It should be noted that the contours of Fig. 1 constitute evidence that there is no one-to-one correlation between the attributes of sensation and the dimensions of the stimulus. Hue, like brilliance and saturation, depends upon both the wave-length and the intensity of the stimulus.[5]

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## THE TENDENCY AMONG CHILDREN TO AVOID WORDS WITH UNPLEASANT CONNOTATION

While analyzing 21,800 remarks obtained from 436 children varying in age from 5.5 to 9.5 yr., the writer was impressed by the predominance of words of a pleasant over those of an unpleasant connotation. The actual ratio of occurrence is 1057 to 80. The material was recorded under a standardized situation, and the subjects were selected so as to be representative of the Minneapolis–St. Paul population.[6] Every child contributed 50 remarks to the total of 118,004 running words and 2033 individual words.

'Pleasant' terms were also found to be more numerous than 'unpleasant' terms on older word lists. The only 'unpleasant' term listed by Horn is *bad*,[7] which is found 89 times, while the 'pleasant' words have 5072 occurrences. Eleven 'pleasant' terms

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[1] D. M. Purdy, Chroma as a function of retinal illumination, *Harvard Univ. Thesis*, 1929, 149 ff.

[2] The data for the intensitive shift from 400 to 20 photons have been published by L. T. Troland, *Principles of Psychophysiology*, 2, 1930, 169-171; and the data for the shift from 1000 to 100 photons are shown in D. M. Purdy, Spectral hue as a function of intensity, this JOURNAL, 43, 1931, 541-559, esp. 547-549.

[3] Purdy, *op. cit.*, this JOURNAL, 553 f.

[4] For instance, these differences are small with respect to the individual differences found for the three spectral primary colors; cf. Hans Westphal, Unmittelbare Bestimmungen der Urfarben, *Zsch. f. Sinnesphysiol.*, 44, 1910, 182-230, esp. 182-187.

[5] This is the argument of E. G. Boring, Isochromatic contours, this JOURNAL, 49, 1937, 130-134. Professor Boring there plots my curve that passes through 525 mμ at 200 photons, and it was he who asked me to publish these complete data because of their direct bearing upon this systematic point.

[6] E. A. Davis, The development of linguistic skill in twins, singletons with siblings, and only children, *Univ. Minn. Inst. Child Welfare Monog.*, 1937 (no. 14).

[7] M. D. Horn, The 1000 words most frequently used by kindergarten children, *Childhood Educ.*, 3, 1926, 118-122.

appear among Thorndike's first 500 words,[1] but only two 'unpleasant' terms have this frequency. Hunt reported an earlier appearance of terms which she characterized as "hedonic."[2] She consulted 61 published children's vocabularies in order to determine the order of appearance of *good, like, love,* and *pleasant,* as compared with *bad* and *unpleasant. Good* was used by children under 2 yr. of age, *bad, like,* and *love* at 2 yr., *pleasant* at 3 yr., and *unpleasant* not until 5 yr. This indicates an advantage for 'pleasant' words in order of appearance as well as in frequency of use.

As a check on these findings, a similar tabulation was made for words descriptive of size and number. Twelve of these expressed bigness, with a total of frequency of 1074, and 6 expressed smallness, with a frequency of 754. Eight of the 'big' words were in Thorndike's first 500, 2 in the second 500, and the remaining 2 were in the first half of the second thousand. Four of the 'little' words were in the first 500, 1 was in the second 500, and the remaining 1 was in the second half of the second thousand. Horn's list contains 7 'big' words with a frequency of 4529 and three 'little' words with a frequency of 3458. Although the number of 'big' words seems to be consistently greater, there is no such disproportion as was found in the 'pleasant' and 'unpleasant' terms. There seems therefore to be a distinct possibility that children tend to avoid terms with an unpleasant connotation.

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## THE NUMBER OF ARTICLES OF PSYCHOLOGICAL INTEREST PUBLISHED IN THE RUSSIAN LANGUAGE

In the October, 1936, issue of this JOURNAL,[3] Professor S. W. Fernberger tabulates the number of articles of psychological interest published in the different languages in the 10-yr. period 1926-1935. The tabulation is based upon articles listed in the *Psychological Index,* and the percentage of Russian articles accordingly ranges from 8.1 in 1925 to 3.8 in 1935, with a mean of 6.78%. As a number of readers will no doubt consider this Russian percentage to be the true ratio of psychological research published in the Russian language, I would like to note that a recent check by me of a few topics over several years showed the *Index* to contain only about 30–60% of the total number of Russian psychological publications. It is my estimate that no less than 15% of the world's psychological literature is now being published in the Russian language and that this percentage may be as high as 20–25%.

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## Carl Stumpf: 1848–1936

Carl Stumpf died on Christmas day, 1936, in his eighty-ninth year and the last important human link with those early decades of experimental psychology, which produced Wundt, Müller and Ebbinghaus, was broken. It was a great period in

[1] E. L. Thorndike, A teacher's word book, *Teach. Coll. Bureau Publ.* 1931, 1-182.
[2] E. B. Hunt, The genetic primacy of hedonic terms, this JOURNAL, 44, 1932, 369-370.
[3] *Op. cit.,* 48, 1936, 680-684.

which virgin soil was tilled and Stumpf had an important part in guiding the plow and sowing the seed from which our present day psychology has developed.

Stumpf was preëminently a scholar of profound erudition but it was not so much his knowledge which impressed his students as his critical ability and keen logical thought, which he felt he owed to the tutelage of Brentano. It may be due to his emphasis in the laboratory upon method rather than upon substance of thought that he inspired no disciples to carry on his doctrines. At his seminar he taught his students by his example to think clearly, to present a thesis with care and honesty, and to argue with logical precision. Lack of knowledge he would forgive but faulty deduction would either irritate him and provoke a sharp reply or would be entirely ignored. Yet he was ordinarily a very kindly man and, so far as the academic dignity of a German university would allow, he showed a generous attitude toward his students. He was by nature reserved, sensitive and somewhat shy and had that modest simplicity of manner that is characteristic of a truly great mind. Physically he was slim, straight and strong and walked with long strides and a springy gait, born of much mountain-climbing. In his speech, as in his writings, his words were carefully chosen to express his exact meaning and he defended his ideas with eagerness, vigor and directness. In disputes he always tried to be fair and he intensely disliked *ad hominem* arguments, as is exemplified by his withdrawal from an altercation with Wundt after the latter had shown undue animus.

Carl Stumpf was born on April 21, 1848 in the small Bavarian village of Wiesentheid. His father was physician to the district court. A number of his immediate ancestors on both sides of the family were scientists and scholars. His early intellectual development benefited particularly from the guidance of his maternal grandfather, who, after his retirement as official physician, came to live with the family and helped in his education. Both of Stumpf's parents were musical and he soon showed his great love for this form of esthetic pleasure. At the age of seven he began to play the violin and at ten he composed an oratorio for three male voices. It is significant for an understanding of Stumpf's dominating life interests that he had this combination of scientific and musical influences during the formative period of his life. According to his own confession, when he entered the University of Würzburg at the age of seventeen, his love for music was greater than for science and he found hard thinking irksome. If he had not lived in an intellectual atmosphere and had not been nurtured by a scientific tradition during his boyhood, it is fair to predict that Brentano, who came to Würzburg during Stumpf's second semester, would not have been able to train him to control his emotional nature and submit to the laws of reason, in which case he might have left us a tone picture instead of a tone psychology.

Brentano influenced not only Stumpf's reasoning power but perhaps even more profoundly the content of his thought. At this period he aroused Stumpf's interest in ethical and religious ideals and at the same time taught him the value of a scientific approach to philosophy, a lesson which became the guide to much of his future thought. At Brentano's advice, Stumpf went to study with Lotze at Göttingen. The effect of Lotze's teaching likewise can be traced in much of Stumpf's later work. Of the two men, Brentano and Lotze, I have the impression that he had the greater respect for Lotze although he was more influenced by Brentano's doctrines. Perhaps it is of some slight significance that the book he most strongly recommended to me for my own development was Lotze's *Medicinische Psychologie*.

Stumpf received his Ph.D. in 1868 with a philosophical dissertation, then returned to Würzburg where he continued his interrupted work under Brentano and began to study theology with the intention of entering the priesthood. He soon found, however, through the persuasion of Brentano's logically consistent ideas, that he could not accept the dogmatic theology of the Roman Catholic Church. He therefore returned to Göttingen, where in 1870 he obtained an instructorship. He was now definitely headed toward a career in philosophy and psychology. There he met E. H. Weber and Fechner and served as subject in the latter's famous experiment on the golden section.

During the next two decades Stumpf changed his academic affiliations a number of times. In 1873 he returned to Würzburg as full professor; two years later he started his *Tonpsychologie*. In 1879 he accepted the chair of philosophy at Prague. Here he had as colleagues Marty, Mach and Hering. In 1884 he was called to Halle and in 1889 to Munich. At Munich he was able to obtain a few pieces of apparatus, including an old tuning-fork piano, and he started his first experimental laboratory—if a small collection of instruments can be called a laboratory. At least he was enabled to make sufficient acoustical observations to complete the second volume of his *Tonpsychologie*.

Finally in 1894 he gained the most coveted chair of philosophy in Germany. He was appointed professor at Berlin and his wanderings came to an end. Ebbinghaus was there as an associate professor and Stumpf found a seminary of three dark rooms, which expanded into an institute consisting in 1900 of the top floor of an apartment house on Dorotheenstrasse. There were several rooms for experimental purposes, a large seminar room and a library where in memory I vividly see Von Hornbostel, Wertheimer, Köhler, Koffka, and Gelb in heated discussion of Von Ehrenfels' theory of *Gestaltqualitäten*, laying (what they then little suspected) the cornerstone of their configurational psychology. Stumpf remained mostly secluded in his office. An interview was a rare privilege. Schumann was his first assistant, followed by Von Hornbostel and Ach for brief periods and then by Rupp. Stumpf discovered early in his career that he lacked manual dexterity and he left all the details of the experimental set-up, including his own, to his assistants. It was a rare occasion when he appeared in the students' research room. For example, he only once made an observation in my visual experiment and then it was a hurried five-minute visit motivated by anxiety over the unorthodox results I was obtaining. Yet one realized that he felt his responsibility for the work of his laboratory, especially when he began to criticize the final thesis. It was in the seminar, however, that his students gained the most from him. In 1920 the laboratory was moved from the rather gloomy and malodorous apartment to twenty-five rooms in the imperial palace. Experimental psychology had finally an abode more fitting to the dignified status to which Stumpf and his assistants had brought it. In 1921 he retired officially from the university and was succeeded by Wolfgang Köhler.

Stumpf was very productive during his years in Berlin. He was also drawn into many outside activities. He was joint president with Lipps of the Third International Congress of Psychology at Munich, and there delivered his address on the relation of mind and body, in which he defended his interaction theory particularly against the argument of the conservation of energy. In 1900 he helped found the *Verein für Kinderpsychologie* and delivered an address *Zur Methodik der Kinderpsychologie* at the first meeting, in which among other things he defined the earliest fear reactions

as reflexes to which the idea of danger comes later through experience. He also criticized the growing tendency in America to use data from questionaries. Notwithstanding Stumpf's interest in the *Verein*, however, he was not practically concerned with child psychology, except in the case of musical prodigies. In the same year he helped found the *Phonographische Archiv*, a collection of records of primitive music gathered by missionaries and travellers and studied principally under the direction of Von Hornbostel. In 1904 Stumpf investigated "der kluge Hans" with the help of Pfungst, who devised the ingenious experimental settings.

In 1907-1908 he was rector of the University of Berlin and delivered as his inaugural address *Die Wiedergeburt der Philosophie* in which he clearly indicated the direction of his philosophical thought. He expressed himself firmly against idealism and mysticism, which latter he thought should be left to the poets. Philosophy, he argued, should be the helpmate of science, and philosophers should engage in some manual work in order to obtain direct concrete experience. He praised Leibnitz for his understanding of the progress in mathematics and science and for his great influence on the development of the latter. In his speech *Vom ethischen Skeptizismus*, delivered in 1908, he expressed the highest ethical idealism. For him moral behavior is characterized by insight as distinguished from mere instinct or habit, but an insight founded upon and thoroughly impregnated with knowledge. The individual should sacrifice his immediate interests for those of humanity, but his moral sentiments should have an intrinsic value independent of external command and force.

During his last years he lived in retirement, saddened by the unfortunate world situation. In one of his last letters he stated that he was not a good enough philosopher to maintain a complete stoicism toward existing conditions, yet he was endeavoring to cultivate that attitude. He was mentally active until his death and was engaged in an extensive epistemological work which he had hoped to bring to completion.

Stumpf, in contrast to Wundt, did not write easily. He published a large number of pamphlets but few books. He never compiled a systematic psychology; his doctrines must be gathered from his various publications. He gave intensive thought to his theories and every sentence he wrote had definite significance. It was difficult writing and perhaps that was why he did not produce more. When once he had put down an idea he rarely changed it in future treatises, so that the contents of his various publications are surprisingly consistent.

His first book, *Über den psychologischen Ursprung der Raumvorstellung*, 1873, is a remarkable book for a man of twenty-five, and it was prophetic of a brilliant career. In it he defended the nativistic theory of space perception with great skill, and in a style of argument and counter-argument which he had been taught by Lotze and which he retained throughout his life. Muscle sensations for him remained muscle sensations and, as he said in his lectures, local signs were like addresses on letters. When the sensations and local signs arrived at the higher centers, how could the mind know where to place them any more than a postman, without knowledge of his route, could deliver his letters?

The *Tonpsychologie* was his outstanding work. The first volume was the nearest approach he ever made to a general psychology. Here one finds his psychophysics and his ideas on such general topics as attention, analysis and comparison. His famous contribution to a theory of consonance and dissonance appeared in 1898. He was much interested in the nature of probability and wrote an article in 1892 directed

principally against Von Kries' theory. He also wrote on the emotions and formulated a theory of feeling as sensation, which I believe he based principally on his experience of extreme pleasure and pain produced by tones.

Two of Stumpf's most important papers were *Erscheinungen und psychische Funktionen* and *Zur Einteilung der Wissenschaften,* published in 1907. In them he presented his fundamental concepts of a systematic psychology based upon but not entirely agreeing with Brentano's teachings. He separated function from content, the act of hearing from the tone heard. The principal empirical reason for this separation, he believed, was that the two, function and content, can vary independently. The study of function, he contended, belongs to psychology, that of content to phenomenology. That this distinction was purely academic is shown by the fact that Stumpf never considered himself a phenomenologist although his work was almost entirely in that field. In 1918 he published an interesting paper on the relation of sensation and image entitled *Emfindung und Vorstellung.* At the same time he also published *Struktur der Vokale,* a subject in which he was much interested during his later years. The publications I have mentioned are the highlights in Stumpf's career. Space does not permit a more detailed account of his writings.[1]

"Theorien kommen und gehen, aber ich habe in meinem Leben doch einige gute Beobachtungen gemacht," quoted Stumpf on his seventy-fifth birthday. Observation for Stumpf was as important for a study of mental life as was experimentation. This attitude was probably more a matter of temperament than actual conviction, for he had the greatest respect for rigorously controlled laboratory research, and always encouraged his pupils in their experimentation. He never entirely outgrew the philosophical approach which he gained from his masters, yet he was primarily a psychologist in that, whatever subject he touched, he almost invariably directed his inquiry toward some aspect of mental functions. It is in the development of modern psychology that his influence has been great.

Princeton University H. S. LANGFELD

## Herbert Nichols: 1852–1936

Herbert Nichols, born at Walpole, N.H., on February 7, 1852, died at Brookline, Mass., on December 6, 1936 in his eighty-fifth year. He was graduated from Worcester Polytechnical Institute at the age of nineteen years. After sixteen years of service in the engineering department of the Pennsylvania Railroad, he entered Clark University on its opening and in 1891 he obtained his doctorate in psychology under G. Stanley Hall. An instructor in psychology at Harvard for three years and a lecturer at Johns Hopkins for one year, he finally returned to engineering, and for his achievements in that field his name was starred in the first edition of *American Men of Science.*

[1] For a comprehensive account of Stumpf's life and works, see E. G. Boring, *A History of Experimental Psychology,* 1929, 351-361, 370 f., and Stumpf, in *A History of Psychology in Autobiography,* edited by Carl Murchison, 1930, I, 389-441. For a description of Stumpf's influence on psychology in general and especially on that of the University of Munich, see R. Pauli, Psychologie der Neuzeit mit besonderer Berücksichtigung des Psychologischen Institutes der Universität München, *Arch. f. d. ges. Psychol.,* 93, 1935, 520-570.

During Dr. Nichols' short excursion into psychology he wrote one book[1] and sixteen articles. His doctoral dissertation, *The psychology of time*,[2] is a pivotal study of that topic. All the earlier work in this field he reviewed, criticized, and evaluated; and he gave direction to the studies that have since appeared by his own experiments. In his articles on pain and feeling,[3] he combated the view of his day and generation that pain is a common feeling aroused by excessive stimulation of any sense-organ. Antedating Von Frey, to whom the credit is usually given, he ascribed separate nerves to pain and claimed for pain the status of an independent sense-modality.

Dr. Nichols was a man of distinct promise in psychology. That he does not now rank among the great in our science is because he chose distinction in engineering.

K. M. D.

## Florence Richardson Robinson: 1885–1936

Florence Richardson Robinson died in New Haven, Conn., on December 3, 1936. She took her Ph.D. at the University of Chicago under Angell in 1908 when Watson was still there. Her thesis on sensory control in the white rat, because it supplemented Watson's finding of the importance of the rat's kinesthesis, was significant at that time and is still cited. It was also one of the early papers to use the jumping reaction of the rat. Her other most important contribution to psychological research is her study (1923) of a case of yellow-blue color-blindness. She was professor of psychology at Drake University from 1908 to 1919, and assistant professor of psychology at the University of Chicago in 1920-21.

In 1921 she married Professor Edward S. Robinson, then at Chicago, and later at Yale. Since that time most of her work has been fused with his. They have published several joint papers of importance, and the demand for their *Readings in General Psychology* (1923) took it into a revised edition (1929). It is well known how she worked with her husband on the *Psychological Bulletin* when he was its editor, and on the accounts of the *American Psychological Association* when he was its treasurer. All in all her contribution to American psychology has been much greater than the formal record shows.

Wesleyan University — JOHN A. MCGEOCH

## An Acknowledgment

The JOURNAL is indebted to Professor C. A. Ruckmick of the Department of Psychology, University of Iowa, for the photograph and signature of Carl Stumpf that are reproduced in the frontispiece of this number.

K. M. D.

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[1] *Our Notions of Number and Space*, 1894, 1-201.

[2] This JOURNAL, 3, 1891, 453-524; 4, 1892, 60-112.

[3] The origin of pleasure and pain, *Philos. Rev.*, 1, 1892, 402-432, 518-534; Pain nerves, *Psychol. Rev.*, 2, 1895, 487-490; The feelings, *Philos. Rev.*, 4, 1895, 506-530; Pain nerves, *Psychol. Rev.*, 3, 1896, 309-313.

# BOOK REVIEWS

*L'analisi elettroacustica del Linguaggio.* By A. GEMELLI and G. PASTORI. Milan, Publicazioni della Universita cattolica del sacro Cuore, serie sesta, Vol. VII, 1934. Vol. I, pp. viii, 250; Vol. II, atlas of 88 tables.

Students of every branch of the science of languages, singers and teachers of vocal music, as well as psychologists and physiologists, will feel indebted to Professors Gemelli and Pastori for this fine experimental study of the structure of speech and its relation to perception and thought. In this coöperative undertaking, Gemelli, a psychologist, is responsible for apparatus, technique, and psychological interpretations, while Pastori is responsible for physiological explanations, and the mathematical analysis of the graphs or oscillograms which correspond to the acoustic forms studied. A large number of oscillograms are reproduced in the 88 tables of the second volume enabling the reader to verify the author's interpretations at will.

In the first chapter there is a historical review and detailed discussion of indirect methods of analyzing language, by the conversion of vocal sounds into mechanical or optical phenomena, registered graphically, and analyzable at leisure. Cogent reasons are offered for preferring oscillographic registration to other forms.

The oscillograph, described in Chapter II is an electro-mechanical device by means of which the electric disturbances, produced in the microphone by sound waves, operate an oscillator equipped with a small mirror, causing the rapid deviation of a ray of light as it plays upon a rapidly moving photographic film. In this way the most minute variations in acoustic structures may be registered in the form of a permanent visible oscillogram. The instrument employed in these researches was equipped with two additional oscillators of lower frequency to record time in milliseconds (m.s.) and for the registration of throat and lip movements. The microphone was located in a soundproof room, at a distance from the oscillograph, and the sounds were conducted separately to a loud speaker in the interest of auditory control.

From the analysis of oscillograms it is possible to discover the characteristic properties of individual vowels and consonants as well as the changes they undergo when in combination with other letters, *i.e.* in words or syllables. The duration of phonemes, and the relation between melody of phrase, intensity and expressiveness is also open to this method of investigation. Differences in the language of monologue and dialogue, in the vocal forms of affirmation, negation, interrogation and exclamation, as well as in characteristic emotional situations may be readily traced. In short the method permits the registration of the subtlest differences in acoustic forms.

Chapter III is devoted to the structure of vowels, isolated as well as in combination, and the differences between spoken, sung and whispered speech. The oscillograms show that every vowel exhibits three distinct phases, the typical vibration characterizing the middle or most uniform phase. In combination with other vocal elements transitional phases of distinctive character appear. Three types of sound waves are observable: (1) *complex periodic waves* of great amplitude corresponding to deep vowels; (2) *aperiodic waves* of slight amplitude with higher frequency and duration, characteristic of the expirational vowels; and (3) periodic waves of still

greater amplitude and duration, but complicated by cycles in amplitude and form. Of the vowels, *u* possesses the simplest structure consisting of two sinusoidal waves, one of great amplitude, the other of less amplitude and double frequency. It also possesses great structural plasticity and fuses readily with adjacent sounds.

The vowel *i* is more complex involving two groups of waves: long and slow, and short and frequent, either of which may vanish in combinations resulting in considerable variability.

The structure of *a* is most complex of all. It is characterized by the presence of overtones of greater amplitude than that of the fundamental or first harmonic. The resultant periodical interferences produce the peculiar rich and resonant quality of this vowel. Of the vowels, under abnormal conditions of voice production, *a* is the most stable and the furthest advanced from a genetic standpoint.

The vowels *e* and *o* resemble *a* in general structure and may be called intermediate forms, the former representing a transition between *a* and *i*, the latter a transition between *a* and *u*. Every vowel has a unique individual structure and a comparison of these structures, will interest the genetic psychologist in search of the probable order in which the vowel sounds were evolved.

Chapter IV treats of the structure of consonants, a more difficult problem because of the brief duration, numerical variety, and linguistic variability, in this class of elements. Consonants, like vowels, however, show a characteristic structure and may be readily identified from the graphs. The results obtained lead to an objective classification of consonants into pure and impure sounds, and persistent and instantaneous noises (rumori).

In Chapter V the authors investigate the minimal duration of recognized sounds. It appears that frequency of vibration is more important than duration in determining recognition. In order to perceive a vowel correctly, two typical vibrations corresponding to a duration of 1/130 to 1/245 sec., suffice. Duration decreases with increasing pitch. Gemelli refers the facts to the "constancy phenomenon," observable in several sensory fields, and finds support for his general theory of intuitive organization.

The next chapter, Chapter VI, deals with timbre in individual voices. Voice timbre is the totality of elements which enable the listener to distinguish one voice from another, independently of the nature of the phoneme and the frequency and intensity of its component vibrations. Individual differences in the pronunciation of vowels contribute most of the characteristics of voice timbre, the more silent consonants producing little influence. Voices are recognized through deviations in the fundamental of typical vowels, variations and interrelationships of amplitude and harmonic phase, and small changes in cyclic oscillations were present. The graphs yield individual voice profiles which are uniform for each subject, and in this way the likenesses and differences between voices as well as their normal variations may be determined with great exactitude.

The final chapter is perhaps of most importance to the psychological theorist since it presents Gemelli's conclusions as to the relation of language to the general function of expression. It is significant that Gemelli, after having elaborated an unsurpassed technique for the discovery of vocal elements, assumes a configurational point of view in the evaluation of his results. Elements, he believes, form no useful point of departure for conclusions concerning language as a function of the organism

as a whole. The elements indeed are only significant in their relation to the whole. Elements are always relative to their functional rôle in the developing form of living speech, which obeys the general laws of the psychic organization of relative materials into unified meanings.

The work is abundantly provided with diagrams, tables and summaries of experimental findings and theoretical conclusions, making reference an easy task. The possibilities of this method and technique of investigation are almost unlimited and the results might prove very important to applied psychologists in several fields.

University of Cincinnati CHARLES M. DISERENS

*Psychology.* By GLENN D. HIGGINSON. New York, Macmillan Co., 1936. Pp. xiii, 646.

This book, written as a text for the elementary course, opens with a general introductory chapter on the nature of psychology, its methods, applications, and its relation to other sciences. This is followed by two chapters on the determination of activities, which touch upon many of the problems of heredity and environment and of motivation. Six chapters on perceptual (and sensory) phenomena follow. The next three chapters deal with action patterns, their development from the random mass action of the infant, their dependence upon the nervous system, and special problems such as hypnosis, choice, and will. One chapter is then devoted to each of the following topics: memory, imagination, thinking, emotion, learning, individual differences, and personality. Each chapter is preceded by a topical outline of its contents. References are given at the end of each *topic,* a procedure which introduces a certain amount of confusion, especially since the reader is told in the opening pages that such references will be found at the end of the chapter (p. 11).

The author lays much stress throughout the book upon the individual's 'personal history" and present environment as determinants of behavior. In Chapter II, for example, are to be found a lucid discussion and vivid illustrations of the environmental determination of many forms of behavior commonly called "instinctive." It is rather surprising, therefore, to find that the rôle of environment and personal history is greatly minimized in the discussion of intelligence. Thus, in the chapter on "individual differences" (which is really a chapter on intelligence), the greatest weight is put upon hereditary factors. In this chapter, and in other parts of the book in which intellectual differences are mentioned, there appear also a number of rather loose or inaccurate statements. In discussing sibling correlations, for example, the old argument is cited to the effect that sibling correlations in mental and in physical traits are very similar, and since the latter are very largely independent of environment, the former may also be so regarded (p. 598). This argument has frequently been criticized in the literature and is entirely fallacious. As an illustration of the hereditary determination of intelligence, Sequin's attempt to train the wild boy of Aveyron is cited as "wholly fruitless" (p. 67). This is misleading, insofar as Seguin himself reports a remarkable degree of progress in his subject, and insofar as training was begun relatively late in life. Higginson also seems to give somewhat too much credit to Freud for calling attention to the contribution of personal history in the development of the individual. The influence of past experience and of the reactional biography of the individual has been repeatedly stressed

by many behaviorists and other psychologists whose writings antedated psychoanalysis.

The topic of learning might profitably have been discussed earlier in the book. It would then have been possible, for example, to explain more clearly *how* the personal history of the individual may affect the development of perceptual phenomena and of action patterns. In the chapter on learning, a very narrow view of conditioning is presented. The term conditioned *reflex* is employed, rather than the more general current term, conditioned *reaction,* and the process of conditioning is discussed only in terms of bodily functions and internal processes.

In a number of instances, terms are employed rather loosely in regard to the rôle of the nervous system in behavior. Thus, thinking is described as "a function of the brain" (p. 461). In another connection, we are told that: "Intelligence, therefore, is what the brain does" (p. 574). Since, as the author himself points out (Ch. XII), our present state of knowledge makes any explanation of psychological phenomena in terms of the nervous system very speculative, such statements will create a false impression in the uninitiated reader.

Finally, it seems that the book might have gained by the inclusion of more experimental material and fewer popular illustrations. To be sure, the author clearly states in the preface that the latter were not intended to "prove" anything, but are only examples. The prolific use of vivid illustrations doubtlessly makes the book more interesting to the beginner, and serves to put the idea across. The question can be raised, however, whether in addition to teaching the facts of behavior, the beginning course should not foster a scientific attitude towards such phenomena and stimulate a healthy skepticism toward uncontrolled everyday observations. The latter would imply a familiarity with the methodology of psychology, which can only be obtained by the examination of typical experiments on each problem. A second point to be considered is that the student, encouraged by the practice followed in the text, could cite many similar instances from *his* everyday observation which may contradict the conclusions of psychologists. It should probably be one of the functions of the introductory course to instill in the student the need for controlled conditions and to discourage the all too common tendency to cite "examples."

Barnard College, ANNE ANASTASI
Columbia University

*Enjoyment of Laughter.* By MAX EASTMAN. New York, Simon & Schuster, 1936. Pp. xviii, 367.

Max Eastman's study of the anatomy of humor must be evaluated in the light of the three disparate functions fulfilled by the volume: (a) as a vehicle for presenting his theory of humor; (b) as a medium for expounding and illustrating his theory regarding the writing of text-books; and (c) as a repository for numerous examples of the stuff of which lasting humor is made.

Disposing of these in reverse order, we may note that the volume is profusely illustrated by verbal and graphic samples of the humor of a century. Artemus Ward, for all practical intents and purposes, sets the earlier limit of a sampling which includes such contemporary contributors as Soglow and Thurber. There is no dearth of illustrative material, and the present reviewer would rarely demur at the selection

of any one of the hundreds of items presented in support of the author's theory of humor.

Nor is the author's conception of a good text-book undeserving of serious academic consideration. Eastman has long believed, he says, that "all textbooks are wrongly written. . . . The mind should approach a body of knowledge as the eyes approach an object, seeing it in gross outline first, and then by gradual steps, without losing the outline, discovering the details." This plan of development the author follows scrupulously, presenting his general thesis in terms of four 'laws' on the single page which constitutes his first chapter, and then gradually expanding his exposition of these principles through the balance of the volume. Eastman makes a good case for this method of writing a textbook. His presentation at the outset of the position he proposes to defend adds both to the interest and to the comprehensibility of an excursion through the later pages. One feels that his early confession of faith served to limit and to direct the author as he proceeded through the formulation of the exposition. At the same time, one wonders if the application of Eastman's principle would not improve many textbooks current in psychology. There is even a suspicion that the necessity of stating at the outset precisely what one expected to contribute might have eliminated altogether some proportion of current psychological publication!

The author presents his theory of humor in the form of four basic 'laws.' (Since certain contemporary psychologists have not hesitated to apply the term 'law' to any generalization they have seen fit to propound, regardless of the degree of demonstrability, this reviewer could scarcely be justified for taking Mr. Eastman to task for his use of the term in a context no less defensible.) These four general principles may be summarized as follows: (1) Things can be funny only when we are in fun. (2) When we are in fun, pleasant things are still pleasant, but things not too unpleasant may also become funny. (3) Being in fun is most natural to childhood; any theory which ignores this is *ipso facto* inadequate. (4) Adults vary in the degree to which they retain the aptitude for being in fun.

Although the author writes no small part of his exposition with his tongue in his cheek, he is in deadly earnest about these basic principles. He lives up to his early promise to make them the skeleton of his entire essay, and they become the frame of reference for his interpretations of the psychological literature of humor. They are the instruments with which he assays philosophical and psychological theories of humor, and they provide the ultimate criteria for the formulation of his 'ten commandments' for evaluating the humorous.

The volume naturally divides itself into five main sections (although these are not the author's divisions). In the first, Eastman develops his four basic principles, laying much stress on studies of infant laughter as a key to understanding adult laughter. In the second, he distinguishes two major categories of humorous materials: *practical jokes*—which involve a line of development undergoing a sudden change of direction; and *perceptual humor*—the apprehension of a funny situation. In the third section Eastman discusses types of humorous materials from cartoons and puns through hyperbolic tales and risqué stories. In the fourth, he presents his 'ten commandments' of the comic arts, and in the fifth, a curious melange of opinions on the nature of humor, contributed by those who make their living directly or indirectly through humor. Eastman at this point has already devoted more than 300

pages to a well-illustrated exposition of his theory; one wonders how much he gains by adding to this citations from testimonials provided by professional humorists, even if the list include such savants as Mae West, Groucho Marx and Joe Cook.

Because the volume should serve to provoke, rather than to block research, the author may well be excused for the occasional moments of elephantine playfulness he exhibits in citing psychological researches or in dealing with psychological theories of humor. He has performed an important service by presenting a thoughtful attack upon any theory which would predicate all humor upon one motive, whether that motive be regarded as a quest for superiority, an escape from inhibition, or whatnot.

Cornell University. JOHN G. JENKINS

*The Psychology of Adjustment: An Objective Approach to Mental Hygiene.* By L. F. SHAFFER. Boston, Houghton Mifflin Co., 1936. Pp. xix, 600.

Although as indicated in the subtitle this book is intended as "an objective approach to mental hygiene," it is useful to others than the special student of mental hygiene. The author suggests the basic nature of the book when in treating "positive mental hygiene," he says "the theories developed throughout this book pertain as much to the psychology of those who adjust well as to those who do so poorly."

Part I, "Psychological Foundations," is a much more thorough and consistent treatment of the basis in man's nature for learning, and therefore for adjustment, and of the process of adjustment or learning itself than is to be found in many textbooks in educational psychology. The treatment of instincts, following the statement that "the theory of instincts has ceased to be significant and useful in psychology, but demands recognition because of its great influence in popular thought," is an instance of the soundness and historical adequacy of the treatment throughout. The conditioned reaction is set forth with clarity as fundamental in the "emergence of a new behavior pattern." The author has done much to promote a simplified psychology of learning by stating that, "a solution of the problem of trial and error learning that avoids the difficulties of both the pleasure-pain and Gestalt theories is to regard such learning as a special case of the conditioned reaction." The reviewer ventures to suggest that this important truth would have been promoted, as it deserves to be, had the author brought together his treatment of these two concepts into a single chapter on adjustment or learning. Some confusion results from his treatment of the "conditioned response' in a chapter on "The Modification of Behavior," and treating 'learning by trial, error, and success' in another chapter under the caption, "Adjustment and Learning." More integration is needed.

In Part II, "Varieties of Adjustive Behavior," the author recognizes that "unfortunately little agreement exists among psychologists as to how adjustive mechanisms shall be named, and even less on the question of classification." Discarding classification on the basis of *kind of person,* and on the basis of *cause,* he adopts a relatively simple classification on the basis of *mode of response.* This gives the following types: (1) adjustment by defense; (2) by withdrawing; (3) involving fear and repression; (4) by ailments; and (5) persistent non-adjustive reactions. Really the last is not a type of adjustive response but the result of failure of all forms of adjustive behavior. The generally accepted mechanisms are treated under the most appropriate of these major heads with sufficient distinctiveness for each

and with enough case studies to make a very useful organization of the material. There is full recognition by the author that such classifications are largely for purposes of convenience in naming.

Part III treats "Personality," defined as an individual's "persistent tendencies to make certain kinds of qualities of adjustment." The bearing of the treatment of personality on the theme of the book is shown in these statements. "How an individual will adjust to the blocking of his motives depends primarily on *what kind of a person* he is when thwarting occurs. Individuals show significant differences in their ability to tolerate thwarting and also in the types of adjustment mechanisms that they habitually employ. These differences have been described as variations of personality." Thus, study of personality in a given instance reveals the need for adjustment, the means of adjustment, and the product of adjustment. Hence its importance in such a book.

Part IV, "Techniques of Mental Hygiene," is sufficiently a unit to merit reading separately by any one wishing a brief treatment of mental hygiene. It should be very helpful especially to parents and teachers.

The "Questions and Exercises for Thinking and Discussion" at the end of the book, arranged by chapters, show care in their making. They are much more than direct review questions on the text. There is a workable index. The book is remarkably free from loose statements, extreme views, and personal bias. The few loose or extreme statements are such as will be readily detected and corrected in a later edition, which is confidently predicted.

This is not just another book to burden the shelves or bore students. There is much well organized content, that will repay careful reading by those who may be expected to know their psychology of learning and by those who presumably are skilled in techniques of directed learning. Those who qualified on both counts will profit by a careful examination of this integration of psychological theory and principle and mental-hygiene practice.

Cornell University PAUL J. KRUSE

*Sex and Personality.* By LEWIS M. TERMAN and CATHERINE COX MILES. New York, McGraw-Hill Book Co., 1936. Pp. xi, 600.

The substance of this book is perhaps better summarized by its sub-title, "Studies in Masculinity and Femininity," than by its title, for it is concerned with the avowed differences in interests and attitudes of men and women (and boys and girls), and the degree with which these are correlated with observable differences in behavioral and cultural tendencies of the two sexes.

The technique used was primarily the questionary. A series of tests or exercises intended to elicit interests, attitudes, and preferences were given to various male and female groups, and on the basis of the responses obtained, masculinity and femininity norms, in terms of test scores, were established. The battery of tests finally evolved consists of the following: (1) a multiple-choice word-association test; (2) an ink-blot test; (3) a range of information test; (4) an ethical and emotional test; (5) a test of interest; (6) a test of opinion; and finally (7) an extrovert-introvert test.

This battery was given to some 1500 subjects of different age, sex and vocational groups, and in addition to a group of 77 passive male homo-sexuals, 18 female

homo-sexuals and 54 delinquent girls. A great deal of statistical work was done to establish both the method of scoring and the validity of the results. The final procedure was to score items + and — , depending on whether they were chosen predominantly by men or women. The algebraic sum of the + and — responses then constituted the subject's M-F rating, a + score indicating masculinity and a — score femininity. Each item was given the same weight, irrespective of the number of items there may have been in the test or of their diagnostic value.

After a few chapters devoted to techniques, a large part of the book is given over to an analysis of the relationships of the masculinity and femininity (M-F) scores to the various factors which may influence them: to physical, personality, and achievement measures; to age, education, intelligence, and occupation. Most of the correlations are low, but many significant and interesting relationships are otherwise noted. One of the most interesting is that both sexes tend to become more feminine as they grow older. This is especially shown by males; for example; the mean score of the general adult population is +36. For male adults in their twenties it is +57, for male adults in their forties it is +39, for male adults in their sixties +10.

The most masculine males are, college athletes (+92) and engineers (+77). The least masculine are clergymen (+8) and artists (+3). Passive homo-sexual males (and it is the scores of these which are most corroborative of the test) make an average score of —28. Amongst women, the least feminine are college athletes, M.D.'s and Ph.D.'s, who have an average mean score of only —13 and —34 respectively. Among the most feminine are women in artistic occupations (—85) and at the extreme end of femininity the dessmakers and domestics with a mean score of —103. Generally the mean scores on the different vocational groups correspond fairly well with the judges' ratings, but there are some surprising discrepancies, as in the case of policemen and firemen, who are fairly low down in the masculinity scale for males with an M-F score of only +28, as against +53 for the average adult male.

It is to be hoped that this scale does not meet the fate of some of the emotional-data questionaries, that is, of being used by people who have no immediate experience with the problem they seek to investigate. One cannot help considering up the possibility of individuals being labeled as homosexuals and inverts on the basis of test scores just as they are now frequently called extroverts and introverts through similar modes of diagnosis. The authors themselves caution against such uses of the test, but it seems worthwhile to repeat it. We must remember that our concepts of masculinity and femininity are far from being definitive, and that in so far as the immediate tests of them are concerned, one's measures, whatever they may be, must to a large extent depend upon provisional hypotheses.

Bellevue Hospital, DAVID WECHSLER
New York, N.Y.

*The Vectors of the Mind.* By L. L. THURSTONE. Chicago, Univ. Chicago Press, 1935. Pp. xv, 266.

Thurstone attempts in this book, to express the wide variety of man's intellectual activities in a simple classification. "It is the faith of all science," he writes, "that an unlimited number of phenomena can be comprehended in terms of a limited number of concepts or ideal constructs" (p. 44). Specifically the basic problem con-

sidered is: Can a table of intercorrelations based on $n$ variables be expressed in terms of $n - i$ (where $i \neq 0$) independent and additive factors? Thus the success of the solution depends upon satisfying the Law of Parsimony and giving factors that have psychological meaning.

In the first chapter the general problem of scientific endeavor is discussed together with the psychological postulates and definitions to be employed. The second chapter continues the statement of the basic assumptions and states the fundamental factor theorem—that the number of linearly independent common factors in a battery of tests is the rank of their reduced correlational matrix. The third chapter describes the centroid method of factoring a correlation table and is a refinement of the methods presented earlier.

In the fourth chapter the method of principal axes is discussed together with Hotelling's "Method of Principal Components." Thurstone has discarded this method because the weights for a test determined by this technique do not remain invariant as the test is shifted from battery to battery. This chapter, like Chapter V—which deals with the special case of Spearman's tetrad, seems to be included to complete the picture of the various investigations made on the general problem.

In Chapters VI and VII the problem of "Simple Structure" is discussed. Stated explicitly, is it possible to find a unique set of coördinate axes which will represent meaningful categories in terms of which the test may be understood? Five different methods are discussed but only the method of maximizing the number of zero factor loadings is recommended. One example of simple structure based on actual data is given in chapter six. The special case where the restriction is imposed, that all the direction cosines of each trait vector be positive or zero is treated in chapter eight, "The Positive Manifold." This is a restriction that may or may not be necessary for psychological truth, but is presented to show the generality of the method and if the restriction is necessary to provide methods of solving the problem. In connection with the problem of the positive manifold it is advisable occasionally to transform the matrix. Methods for securing orthogonal transformations are presented in Chapter IX.

The last chapter, Chapter X, is devoted to the problem of appraising each individual in terms of the primary factors. The problem of predicting an individual's performance in terms of primary coefficients and the test coefficients is also discussed. No examples are given and this problem needs considerable amplification.

There are three appendices: the first is devoted to the calculation of the factors by the centroid method; the second, to finding the roots of a polynomial and the last, to determining the square root of a number by means of a calculating machine.

It is too early to assay the ultimate value of this work. This must await the application of the methods described by Thurstone to a long series of researches by the experimental psychologist.

Fordham University J. W. DUNLAP

*Aphasia: A Clinical and Psychological Study.* By THEODORE WEISENBERG and KATHERINE E. MCBRIDE. New York, Commonwealth Fund, 1935. Pp. xvi, 634.

This volume is the result of five year's labor devoted to a critical survey of early and modern literature on aphasia, and to the results obtained from a battery of psychological tests applied to 234 aphasic cases. Sixty of these cases furnished the

basis for the critical study included in the text. All cases were found in various Philadelphia hospitals. The same tests were applied also to 85 normal individuals.

The chief objectives in the present volume are (1) the establishment of a battery of tests satisfactory for the discrimination and analysis of aphasic disorders; (2) the study of the psychological changes in aphasia; (3) the classification of types of aphasia; and (4) the study of the nature and location of the lesions present in aphasia.

A large battery of tests were employed as a means for getting at the gross and subtle psychological changes that occur in aphasic conditions. These tests and the methods of using them are described in Chapter VI and in Appendix II.

The principal tests employed were: speaking ability; ability to name objects; to repeat words, phrases, and sentences; reading ability (Gates, Gray, Chapman, Thorndike); writing spontaneously and from dictation; computation and arithmetic problems (Stanford Achievement); language intelligence (Kelly-Trabne, Stanford Binet Vocabulary Test, Thorndike Test of Word Knowledge); reproduction of word material; non-language tests. The results of these were interpreted on the basis of the performance of a group of normal adults who were chosen for the purpose.

The data of these tests led the authors to an empirical classification of aphasic types as follows: the predominantly expressive group; the predominantly receptive group; the expressive-receptive group; and the amnesic group.

There follow four chapters devoted to these types respectively. Each of them includes a discussion of the nature of the disorder, a clear cut description of one or more cases, and a summary.

Subsequent chapters deal with cases of brain tumor; a control group of patients with right-sided lesions and no aphasia; apraxia and agnosia; the course of aphasic disorders; reëducation in aphasia; the psychological changes in aphasia; localization; concepts of aphasia, apraxia, and agnosia: a comparison with current views; and finally, a brief general concluding chapter in which the authors point out the most important contributions of their work.

Certain definite statements can be made in conclusion relative to the nature of aphasia. The lesion must be within an area including anterior parts (*i.e.* within the pre-central convolution and adjacent parts of the frontal lobe) and posterior parts (*i.e.* within the post-central and parietal lobes, the superior part of the temporal lobe, and the anterior part of the occipital). How much of the sub cortex is involved is unknown. "The various surveys of the pathology of aphasia would seem to indicate that the lesion must almost always be in the dominant hemisphere and must include the motor, sensory, and temporal zones so as to affect those parts of the brain concerned with motor, kinaesthetic and auditory activities."

These findings are confirmed by the research on which the present volume is based.

Northwestern University. ROBERT H. GAULT

*Criteria for the Life History, with Analysis of Six Notable Documents.* By JOHN DOLLARD. New Haven, Yale Univ. Press, 1935. Pp. 288.

Two chapters are devoted to the introduction and discussion of the criteria; six to their application to recorded histories presented by different schools of psychologists and sociologists; followed by one giving suggestions for the use of

the criteria by teachers; and this by a final chapter on Culture, Personality and Life History.

The book is valuable as a stimulus to the growing appreciation of social psychology as chiefly concerned with the reactions of individuals to the culture traits of the society into which they are born. The method of arriving at the criteria and testing them by the study of typical life history documents is to be commended. To the reviewer, however, the criteria do not seem sufficiently clear, complete, or exact. They are not formulated in a way to impress one with their exact meaning and theoretical or practical value. One cannot readily carry them in mind as he reads a life history, and it is often difficult to tell which one is being applied in a given paragraph. The criteria are:

(1) "The subject must be viewed as a specimen in a cultural series." Good, clear, and almost axiomatic. (2) "The organic motors of action ascribed must be socially relevant." The meaning remains hazy after reading several applications. (3) "The peculiar rôle of the family group in transmitting culture must be recognized." Why of the family only? especially when other institutions are becoming substitutes for the family? (4) "The specific method of elaboration of organic materials into social behavior must be shown." Not clear or precise. (5) "The continuous related character of experience from childhood through adulthood must be stressed." Yes, but why spend pages in pointing out that an autobiography does not begin until a personality is developed? No autobiography can. (6) "The 'social situation" must be carefully and continuously specified as a factor." Yes, but why social only? Why not physiological and other factors in every situation? (7) "The life-history material itself must be organized and conceptualized." Yes, but must it not be done in a way that scientists will recognize as likely to reveal truth? The dramatist organizes and conceptualizes to get certain emotional effects, while the propagandist does so in another way to accomplish his purposes.

The omission of any criteria regarding sources and reliability of facts greatly restricts the value of this or any other criteria of life histories.

Some persons, by reading a case study by Healy and one of Margaret Mead's anthropological studies, would get more help in writing life histories and in judging the value of a particular life history, than by studying and applying these criteria. The book will probably stimulate other work in this field, and ultimately prove useful to all persons dealing with life histories.

Columbia University EDWIN A. KIRKPATRICK

*Educational Psychology.* By CHARLES E. SKINNER and others. New York, Prentice-Hall, 1936. Pp. xxvi, 754.

This is an elementary textbook intended for students of education and teachers. It is the work of twenty-five psychologists from twenty-two different colleges. The psychological point of view is necessarily eclectic. It stresses the aspects of growth and the genetic method throughout. The individual is regarded as a growing and functioning organism; and every effort is made to consider him as a whole personality.

Part I is given to Orientation and Introduction by Skinner. The individual is "the real subject matter of education"; all other "materials, methods and agencies are but means to an end; namely, a person that is socially and individually efficient, serviceable, and happy."

Part II deals with the problems of growth under the following titles: The General Nature of Growth, by T. R. McConnell; The Acquisition of Skills and Knowledge, by J. D. Lawther; Interests, Attitudes, and Ideas, by W. Hartmann; Reflective Thinking or Problem Solving, by J. S. Gray; Expression and Creative Activity, by J. M. Fletcher; Motivation, by M. K. Thomson; The Development of the Emotions, by A. T. Jersild; Social Growth and Character Formation, by F. F. Powers; Personality Development, by P. L. Boynton; Childhood and Adolescence, by E. S. Conklin.

Part III presents the problems of learning as follows: Learning: its Nature, Acquisition, and Retention, by R. A. Davis; The Transfer of Training, by L. W. Webb; Learning the Fundamental School Subjects, by K. C. Garrison.

Part IV is given to Individual Differences and their Measurement under the following chapters: Individual Differences: Their Nature and Causes, by F. S. Freeman; Intelligence: Its Nature, Development, and Measurement, by P. A. Witty; Educational Measurements, by E. A. Lincoln; Subject Disabilities, by E. R. Wood; Teacher Evaluation: Examinations, Grading, and Reporting, by W. J. Gifford.

Part V presents the problems of Adjustment and Guidance as follows: Personality Maladjustments and Mental Hygiene, by R. T. Rock, Jr.; Adjustment of Deviating Children, by J. E. W. Wallin; Abnormal Children and their Treatment, by F. A. Moss; Guidance, by M. R. Trabue; Teaching as Guidance, by A. W. Aleck.

Part VI stresses a manifold view of Educational Psychology under the title, Viewpoints in Educational Psychology, by J. N. Washburne.

The volume is remarkably consistent in emphasis and well illustrated by concrete, workable examples taken from the class-room. While stressing the importance of considering the other factors of the whole personality, none of the writers have under-estimated the significance of their own problems. Skinner in his introduction and Washburne in his summary of view-points have gone far, however, to make the problem seem entirely too simple. In the present unfolding of viewpoints it would seem wiser to let individuals think out some of the conflicts for themselves; although the present work may be a valuable guide to many readers as they approach numerous books with divergent viewpoints and conflicting methodology.

Comprehensive, perplexingly indecisive in parts, this volume presents the numberless problems of educational psychology to the reader's attention and challenges, even dares, him to solve a few of them in his daily workshop.

University of Pennsylvania R. A. BROTEMARKLE

*Statistics for Students of Psychology and Education.* By HERBERT SORENSON. New York and London, McGraw-Hill Book Co., 1936. Pp. viii, 373.

Sorenson set out to write a book "for use as an elementary and intermediate text," and we may surely grant his success in attaining this aim. The book is admirably suited for the introduction of students with a meager mathematical preparation to statistical procedures. The steps in the calculation of various statistical measures are set forth in great detail, and the necessity for these steps is, in most cases, explained in non-technical terms. The elementary student should come out with some understanding of what he must do and also with some commonsense notion of why he is doing it. The latter result is too often completely missed

in texts which are less detailed. As an indication of the limitations of the text we may say, however, that students more sophisticated mathematically will probably find that the text drags, especially in the early chapters. Also the common sense justifications of procedures do not always give much mathematical insight into the problems involved.

The number of methods adequately treated in Sorenson's text is greater than that found in most textbooks at this elementary level. In addition to the various measures of central tendency, variability, and the more common correlational methods, the author presents such methods as Sheppard's Method of Unlike Signs and the Coefficient of Mean Square Contingency. These methods are presented fully enough so that the student should at least understand researches and articles where these methods are used. Probably most important for the general background of the student, however, is the discussion of the Coefficient of Alienation and the relationship between size of correlation and predictability. One may regret only that this topic and its implications are not emphasized more.

It is unfortunate that Sorenson has not included separate lists of formulae but has scattered them throughout the text. When engaged in working out a series of measures, the student must locate by means of the index a number of separate formulae in the body of the text. Yet it would seem to be advantageous that one could use as a reference work for later problems the statistical text with which one is most familiar.

The chapter on sampling and Sorenson's especially excellent and lucid exposition of "unreliability" should, in the opinion of the reviewer, have been inserted as early chapters rather than left until the very last. The author needs such concepts as probable error before he has adequately explained them (e.g. p. 275). More important still, the theory of sampling and the dependent notion of reliability lie at the very core of statistical treatments. If the student is to interpret experimental findings intelligently, it seems more important that he be impressed with this fact than that he learn the actual methods themselves. The central place of the theory of sampling cannot be made more evident than by giving it an early position in the logical structure of the text, and by referring to it whenever a step is made which depends upon these fundamental notions.

Supplemented by such a broad view of the logical structure of statistical theory, and more convenient hand-books and lists of formulae, Sorenson's textbook should give the student a sound training in the ordinary statistical techniques and their interpretation.

Cornell University T. A. RYAN

*Social Psychology.* By ELLIS FREEMAN. New York, Henry Holt & Co., 1936. Pp. xii, 491.

This text is significant from the layman's point of view for it attempts to contribute to understanding of today's pressing social problems. Descriptions of experiments which have meaning only as examples of methodology are absent. Considered are such topics as the individual's construction of his universe, the creation of values, the function of values, impartiality, acquisitiveness, and industrial coöperation. The argument divides into three parts: (1) the individual basis of social phenomena; (2) individual and cultural determination of values; and (3) the psychology of some fundamental social values. Save in the first three chapters

where Professor Freeman is polemically involved in defining social psychology and in laying the ghost of the "group-mind,' most collegiate juniors will find questions considered which originally brought them into psychology.

This appeal to ready-made interests of undergraduates is fortunate. Fortunate, too, from a pedagogical standpoint, is the way in which the author makes cultural stereotypes stand out in such bold relief that a reader can scarcely avoid a critical attitude toward our own culture. Several aspects of the exposition, however, do mar the book's pedagogical value. First of all, the author omits not only methodological discussions, but also the methods whereby he has obtained his answers. To this reviewer it is just as important to show students how to answer social questions scientifically as it is to give the answers. Secondly, the titles of original contributions to the various topics are omitted, so the reader is left with no way to proceed beyond the text. Thirdly, an emotional partiality regarding economic issues manifests itself. The reviewer agrees with the opinion that economic stereotypes are extremely important, but emotional partiality concerning an opinion should not manifest itself in any scientific text.

Professor Freeman's synthesis of the individual and the cultural pattern is an advance in social psychology as it is found in text-books. Once the polemics against the 'group-mind' are passed, the exposition proceeds smoothly on the foundation that cultural stereotypes operate as individual beliefs and habits of behavior. His appeal to anthropological data avoids, as he hoped it would, any tendency to exalt local cultural peculiarities as fundamental human traits. This appeal might also have been employed to equalize the stress sometimes laid upon the first few years of life in the family, but Freeman has chosen to neglect entirely this portion of social psychology.

This reviewer got the impression that the author's thought and point of view were still in transition. Should this be true, one may hope for further synthesizing in a revision, *i.e.* for the integration of familial influences on the social life of a child, of whatever of the factual can be derived from psychoanalysis, and of the rôle of constitutional and structural influences on social behavior into his exposition of social psychology. So would a good book be much improved.

Brown University J. McV. HUNT

*Philosophische Grundprobleme der Psychoanalytischen Psychologie: Eine dialektische Studie.* By ALEXANDER SZALAI. Zurich, Octava Verlag, 1936. Pp. 63.

Sympathetic critiques of psychoanalysis and attempts to relate its formulation to other universes of discourse have begun to appear in the literature. This study is one of the former. Dr. Szalai has considered psychoanalytic formulation sufficiently clear and mature to stand a dialectical examination. Approaching the task from the standpoint of the immanent critic, he sets forth to determine whether the system of psychoanalysis is internally consistent.

The argument is presented under eight headings: (1) remarks on the intellectual setting of psychoanalysis; (2) the uniqueness of the subject-matter of psychology; (3) psychical events and psychical organization; (4) the conscious and the unconscious; (5) the symbol; (6) the significance of drive and reality for mental organization; (7) the subject-object relationship in the light of psychoanalysis; and (8) the categorical structure of psychoanalytic theory.

Mind-body dualism is defended on the grounds that subjectivity has reality.

Mind and subjectivity are equated. The philosophical concept of subjectivity is then limited for purposes of scientific autonomy by hypostatizing the "personal" (*personale*). A personal subjectivity is considered to consist in a closed dynamical system of psychic energy. This pattern of assumptions justifies the Freudian topology of fore-conscious, conscious, and unconscious. Here, so it seems to the reviewer, Dr. Szalai commits the traditional logical fallacy in psychology. By making an entity of subjectivity he is forced to deal with the whole gamut of mental constructs such as psychic energy. That human organisms can observe internal events, and that their reports may be employed as indicators of internal bodily events and of the governing moments of behavior is justified. But these are methodological considerations; they do not justify putting the observations in an existential category comparable with that of organism.

The final chapter on the categorical structure of psychoanalytic theory presents the first systematic organization of psychoanalytic concepts the reviewer has seen. Perceptual psychology is seen to impinge upon psychoanalysis at the level of consciousness; biology impinges where organo-dynamics meet psycho-dynamics in the unconscious and where the somatic dispositions of the individual (phylogenetic) come in as sources of energy. Sociology impinges where the ontogenetic development of the individual begins, which means that the life experiences of the individual are obtained in a human environment. The psychological person emerges from the conflict between phylogenetic and ontogenetic forces.

Psychologists who like to philosophize will find some tantalizing exercizes in verbal logic in these discussions. The systematic organization of psychoanalytic concepts clarifies the issues but does not settle the problem. Since the universe of discourse remains entirely in the subjective, psychologists interested in formulating the relation between psychoanalytic phenomena and corresponding behavioral phenomena will find little to aid them.

Brown University J. McV. HUNT

*Social Psychology.* By RICHARD T. LAPIERE and PAUL R. FARNSWORTH. New York, McGraw-Hill Book Co., 1936. Pp. xii, 484.

The authors, respectively sociologist and psychologist, have successfully synthesized the recent findings in their two fields into a unitary and distinct socio-psychological study. A tersely critical review of theories remote and recent forms the introduction. There follow in detail the usual topics—theory, process, personality normal and psychopathic, communication, leadership, social situation—as "a story" freshly told, "with plot and continuity." Factual material, including bibliographies and copious notes on recent case and experimental studies, is afforded in ample appendices at the close of each chapter.

The stimulating influence of the late Professor Cooley is acknowledged and evidenced throughout the text. His is the type of neo-Platonism employed to explain human *behavior patterns*. It is society that hath made us and not we ourselves. Not by rational thought, or by free will, or by instinctive urge however modified or "sublimated"; not by imitation of whatever ilk, or yet by "conditioning," are we saved from the animal state in which we are born. The "nature-nurture twins" are carefully weighed; but the latter, apparently, bears at least a slight advantage over the former. "Society" must "teach," and, notwithstanding criticism of Tarde, by "models." There is a harking back from the over-used term "conditioned reflex" to

the "safer" concept of "learning." "Men, we shall find, do learn by experience." It is society that shapes, civilizes, *humanizes,* the individual. Yet if the individual runs amuck in his adjustments to slowly evolved standards, it may be that society and not he should modify its attitudes.

Inevitably a treatise on social psychology is a criticism of current morals. The classroom guide in the use of this and other recent texts, observing the changing mores in colleges and out, may inquire: Does society create the individual in its own image; or have the psychologists, from Freud down, been creating society in their own image—the label stamping the man and the times? There is in this text, admittedly, no "thanking God for the Censor." There is, of course, no Censor; no conscience, however come by. Individual responsibility is *nil.* Like the villagers in Dunsaney's *The Blessing of Pan,* "society" dominies, and all mayhap are on our way to the neo-pagan, the neo-primitive. Nevertheless, advertently or inadvertently, this text does afford ample data for the preachment of older-fashioned standards. What have been termed "innate evils" may also have been acquired by social contagion. Room is left in the world for the "neo-virtues."

Something vibrant, ancestral or acquired, will awaken in the younger reader of this text.

Orleans, Indiana SARAH M. RITTER

*Grundlagen der Rassenpsychologie.* By E. VON EICKSTEDT. Stuttgart, F. Enke, 1936. Pp. 164.

This is a theoretical treatment of race psychology which seeks to have as its background the science of anthropology. There is a thoroughgoing survey of the pertinent literature. The author's thesis is that the psychology of race depends on the concept of form. He insists that the problem to be investigated is the relationship of body and mind. Not only is form important but we must seek to understand the interaction of forms, their totality. He further says that we seek to know the nature, evolution, and changes of psychic forms.

The body forms arise out of zoölogical or biological forms. This we get from anthropology. We must, however, eliminate the concept of *volk* (folk or people) for this is a cultural consideration. He insists, furthermore, that we cannot approach the problem from the standpoint of experimental psychology, for this measures the effect of environment. Differences in mental grasp, only, are evident. Such efforts result in a complete fiasco, as seen "in the case of Garth's attack of Race Psychology." Such an attack, he continues, does not reach the racial psyche which is decided by characterological factors, interplay of will, emotion, and instinct with varying strengths and ways of acting. Henceforth, the author eschews experimental psychology as a method of approach and the study of race psychology. The approach which is therefore indicated for race psychology is the one called the characterological-purposive, with auxiliary branches, as psychology of peoples, the theory of types, behaviorism, and psychiatry.

Here the writer, having parted company with experimental scientific psychology, proceeds to describe the aforesaid desirable attack of the problem of race psychology. This review would be too long if we should try to give these here, but the question to be raised is, can we have a scientific race psychology without basing it on experimental psychology? Would an investigation without the scientific (laboratory) attack

be scientific? It certainly would not be "natural" scientific. It would be an approximate only. One may decry experimentation, as the author does, but in doing so he takes himself away from the psychological laboratory and so from actual scientific psychology, and thence the reviewer cannot follow him.

The writer takes solid ground when he says that racial minds, if they exist, are due to inherited forms as they function. It is the function of these which concerns us. But to evaluate these for purposes of comparison we must measure them—and the author of the book feels little sympathy for measurement. How else could we know there are differences unless we measure them? The author feels there are differences but will not abide by the results of a scientific effort to measure these differences. There is no objection to one's taking sides as regards the racial difference hypothesis. He may believe it is true or not true. The reviewer insists that the question be decided experimentally since it is a psychological problem.

In this day of scientific investigation, which means measurement, race psychology must submit itself to this scientific discipline. The day of indefinite comparisons is past.

University of Denver T. R. GARTH

*Wayward Youth.* By AUGUST AICHHORN. Revised and adapted from the second German edition. Translated by Elizabeth Bryant, and others. New York, Viking Press, 1935. Pp. xiii, 236.

This volume was originally published in 1925 by Internationaler Psychoanalytischer Verlag, Vienna, with the title "Verwahrloste Jugend." A second edition was issued in 1931. It is upon this second edition that the present translation is based. This work deals with the application of psychoanalysis to the study of delinquent children and is illustrated by actual case histories from the author's child guidance clinic and training school in Vienna. The original publication was based upon lectures to candidates in the training school of the Vienna Psychoanalytical Society. The translators warn that much of the local color which gave the German work its flavor has been lost in translation but the book remains highly informal in its presentation and thoroughly readable.

*Wayward Youth* consists chiefly of two main divisions. In the first place it draws upon the author's experience with youngsters and presents several interesting case histories. In the second place it presents a psychoanalytical interpretation of these cases. It is apparent that the author has great practical skill in dealing with problem children and this straightforward account of his methods is quite valuable. The psychoanalytical aspects of the book do not seem equally successful, degenerating at times into mere verbalisms. Somehow the practical handling of the cases presented, rather than the psychoanalytic principles enunciated, seems to constitute the meat of the volume. One feels that the solution of the behavior problem was first evolved, then the explanatory psychoanalytic principles evoked. This situation can be understood if the history of the matter is reviewed. Freud in his foreword makes it clear that Aichhorn had considerable experience and success in dealing with delinquent children before he acquired the vocabulary of formal psychoanalysis.

Aichhorn's work is of value as a record of a sympathetic insight into certain specific instances of behavior problems. In addition to its explanatory shortcomings it is subject to the scientific limitations of any work relying upon case histories.

University of Wisconsin KAI JENSEN

*Die Charaktereigenschaften.* By FRANZISKA BAUMGARTEN. Bern, Francke, 1935. Pp. 81.

*Die Dankbarkeit bei Kindern und Jügendlichen.* By FRANZISKA BAUMGARTEN. Bern, Francke, 1935. Pp. 106.

Baumgarten offers in these two pamphlets a contribution to the development of an "objective characterology." Pointing out that characterologies thus far attempted have been written chiefly along abnormal lines, the author attempts in these studies to establish a normal basis for this work.

The first pamphlet is opened with an historical survey in which all the important work from Theophrastus to the present is reviewed. This is followed by a discussion of contemporary efforts in which the work of Ludwig Klages is severely criticized. Baumgarten then chooses the "character-quality," which he defines as hormic energy which directs an individual's active and reactive behavior, as the basis of his "objective characterology." He illustrates his concepts, differentiates between personality and temperament, and then distinguishes between the real and unreal qualities of character. All the German words denoting character-qualities are listed and Baumgarten attempts to classify and to arrange them in their relationships.

The second pamphlet, as its title indicates, is more promising. Baumgarten attempts here to give a morphology of a single character-quality—gratitude (Dankbarkeit). The first part of the book is devoted to an analysis of this quality which, as he believes, is innate and not acquired. The author shows the importance of gratitude in juvenile psychology, and attempts to trace its genesis. The second part gives the results of an experimental study in which four special testing methods were applied in three different ways. The results show that gratitude assumes one of four forms: (1) verbal thanks; (2) thanks with thought; (3) thanks expressed by a wish for further relationships; and (4) thanks as merely a task. The relationship-thanks are, as Baumgarten believes, the most valuable. It appears earlier among girls than among boys.

Baltimore, Md. ERNST HARMS

*Aktualgenetische Untersuchungen an Situationsdarstellungen.* By URSULA MANTELL. Neue Psychologische Studien, edited by F. Krueger, Vol. 13, no. 2. Munich, C. H. Beck, 1936. Pp. 95.

This doctoral dissertation is a contribution to Krueger's theory that all perceptions have "wholeness" as their primary quality. Miss Mantell showed a picture (12 little-known black and white reproductions of famous artists were used) to 36 psychologically trained *Os*, and then (after very short but progressively longer exposure-times) she asked the *Os* for their impressions of its contents.

As her results showed, all the primary perceptions of the pictures yielded the quality of wholeness which was, for example, expressed in terms like "a fight," "a dance," "a couple," etc. The concepts were simple and uncomplicated and they were chiefly conditioned by the "structure" of the picture exposed. The simpler the contents of the picture, the later the formation of the first concept. Concerning the influence of artistic form, Miss Mantell found that the more the object differed from its environment the earlier was it conceptualized.

The *Os* descriptions of their concepts were most frequently reported in "height and brightness." The contents most rapidly conceptualized were not those the artist

wished to represent in his work, but they were those that are most general in every day life. Simplicity and not the artistic quality is the most important factor in the concept of an object of art.

Baltimore, Md. ERNST HARMS

*Infant Behavior.* By MANDEL SHERMAN, IRENE SHERMAN and CHARLES D. FLORY. *Comp. Psychol. Monog.*, 12, 1936, (no. 59). Pp. 107.

Another collection of the simpler items in the behavior of newborn infants. The task here set included "the control of the conditions under which experiments are conducted; the application of constant and measurable stimuli to an adequate number of subjects; and the accurate description of the responses to the various stimuli applied with different intensities." The study extends an earlier publication by the Shermans (1925). The movements include pupillary reflex, ocular and grasping movements, and behavior interpreted as 'pain,' 'pressure,' and 'defense.' Considerable attention is paid to strength of stimulus. More than 300 infants ("the majority . . . less than ten days old") were used.

Many of the simpler movements elicited are found to increase in regularity and adequacy with age and with increased energy of stimulus. More general 'patterns' of behavior are found to be inconstant during the first post-natal days. Indications of 'personality' are therefore scanty and uncertain. Once more it appears that 'good' babies (of the nurses) are inclined to be quieter and less troublesome than 'bad.' The authors stand against the theory of 'mass activity' in newborn man. This reading of the ambiguous evidence is noteworthy just now, for the principle of gradual differentiation out of original 'mass activities' has begun to meet a sobering criticism. It is natural that those whose primary interest and whose methods favor 'reflexes' should join the critics. Our authors easily turn to a progressive 'domination of the cortex' to explain gradual integration. A careful re-reading of Coghill and his experimental followers would be profitable at this stage.

Cornell University M. B.

*Psycho-Analysis for Teachers and Parents.* By ANNA FREUD. Translated by Barbara Low. New York, Emerson Books, Inc., 1935. Pp. 117.

Teachers and parents who have shied away from psycho-analytic literature as presenting too sordid and unwholesome a picture of childhood will do well to read this clear, concise presentation of the principles underlying psycho-analytic theory. This book contains four short lectures that were delivered before the Hort teachers of Vienna. The Hort is an institution that provides care before and after school hours for children of working parents who would otherwise be neglected in the mornings and evenings.

The most important Freudian concepts of the Oedipus complex, and conflicts arising out of faulty early training are outlined briefly. Early childhood is represented as a period of conflict between infantile instincts such as desire for food, pleasure in sucking, and interest in the eliminative processes, and social pressure afforded by parental training. This period is also characterized by strong emotional attachment between the child and one or the other of his parents; usually that with the parent of the opposite sex is stronger. This period is followed by a latency period in emotional development during the early school years wherein

things go smoothly because the child has curbed his instincts in the socially approved way. This period of emotional peace and calm is supplanted by a second period of conflict at adolescence. At this time the parent normally loses his place as his offspring's chief love object to a young person of the opposite sex.

The book is practically free from case histories, and is shorn of all but the bare outlines of psycho-analytic theory. Parents and teachers cannot accept psychoanalysis as *the one* explanation of childhood's aberrations, but certainly they should understand it and accept it as *one* of the explanations for normal and problem behavior.

Harvard University — MARY SHIRLEY

*Students' Guide to Efficient Study: A Manual Based on the Results of Scientific Investigation into the Study Habits of College Students.* By L. W. COLE and J. M. FERGUSON. New ed. New York, Farrar & Rinehart, 1935. Pp. 38.

If the college student is interested, as he generally is, in efficient use of his study time, he will wish to examine this manual. Complete and concise rules are given for planning the student's work, learning to concentrate, reading and studying, note taking, reviews and examinations, memorizing, and developing correct general habits. Following this exposition on how to study there appears a section devoted entirely to the student's previous preparation, in which criteria are proposed for determining whether his preparation fulfills the demands of college life. Suggestions are also given for correcting weaknesses and surmounting difficulties. The authors emphasize that the student must employ the study rules as his individual case dictates, driving hard at his shortcomings and assuming the responsibility for his adequate preparation.

The substance of this manual, derived as the title states from evidence obtained from experimental investigation in psychology, makes the rules authoritative. The average college student has had little or no instruction during his secondary education as to how to study; as a result of the sudden change to the decidedly different routine of college work, he flounders around, spending much more time and effort than is necessary on his studies. He has no idea, for example, that any subject can be made interesting from a knowledge or a practical use of it, and that that interest will hold his attention to the work in hand. The student will find this book a helpful guide in planning his work and studying efficiently, thereby deriving more complete enjoyment from his college career.

Cornell University — LAURA K. ALLEN

*Les jeux éducatifs de Decroly dans l'enseignement des infants anormaux.* By GUNNAR STADIUS. Societas Scientiarum Fennica. Commentationes Humanarum Litterarum. VIII, 1. Helsingfors, Akademische Buchhandlung, 1935. Pp. 245.

The introductory chapters of this book deal with various investigators' ideas concerning the problems of classification and methods of treatment of subnormals, with theories concerning the education and educability of subnormals, with theories of play, and with the rôle of play in the educational process as defined by Decroly. Finally, Decroly's principles concerning behaviour as employed in his didactic theories of education are described.

The following chapters present in detail and systemmatically Decroly's educational games for the pre-school and early school levels, a method which lays par-

ticular emphasis on developing the visual-motor functioning of the individual in concrete and practical situations. The contributions of other educators, Itard, Seguin and Montessori in particular, to the development of Decroly's method as well as their divergence from his method are discussed. The author makes no contribution of his own by way of interpretation or criticism of the ideology and methodology of Decroly.

Trade Extension Classes
New York City

THEODORA M. ABEL

*The Emergence of Human Culture.* By CARL J. WARDEN. New York, Macmillan Co., 1936. Pp. 189.

This little volume, which makes an easy and entertaining evening's reading, is a semi-popular presentation of the story about the evolution of man and his culture from the animal stage. We have too readily accepted the dogma that our civilization has been heading toward greater perfection and desirability. The author warns against the flaccid optimism which amounts to a fool's paradise. Intelligent social planning is necessary if we are not to be caught in the down-drift.

One will rate this essay according to one's attitude toward the policy—which is quite decidedly the rage to-day—of compiling elementary discussions in the field of psychology and closely related provinces which will be attractive to the 'man in the street'—to use a rather threadbare term. Warden says, "In view of such important evidences of genetic kinship, it is small wonder that most scientists have come to look upon man as a mere animal" (p. 2). May we suggest that this sort of statement is coming to have a rather trite sound? And does it not remind us involuntarily of a school primer to remark: "Rapid means of communication and transport are gradually merging into one the several civilizations of to-day"? (p. 176). There is appended a bibliography of 87 titles, all but one being in the English language. The work is illustrated.

Clinton, N.Y. PAUL C. SQUIRES

*Solving Personal Problems: A Counseling Manual.* By HARRISON S. ELLIOTT and GRACE L. ELLIOTT. New York, Henry Holt & Co., 1936. Pp. viii, 321.

"This book considers the ordinary problems which individuals have to meet and discusses the kind of personal counseling which should be an integral part of the responsibility of a teacher, minister, dean, association secretary, or parent. The personality difficulties considered are those about which an individual might do something himself, if he had the requisite insight and skill, or are those for which he needs only the help which a skilled counselor can give. It is intended both for the person who wants help on his own problems and for the counselor who is called upon to give that help" (p. v). We grant that the book can be of value to some people looking for assistance on their problems. We might, however, raise the question in how far the rudimentary discussions set forth in this work could really be of material assistance to the trained counselor. Possibly the most valuable aspect of the volume is to be found in a rather admirable bibliography.

Clinton, N.Y. PAUL C. SQUIRES